SEP 1 0 1928

PERIODICAL ROOM
GENERAL LIBRARY
UNIV. OF MICH.

The Journal of EDUCATIONAL PSYCHOLOGY

Devoted Primarily to the Scientific Study of Problems of Learning and Teaching.

BOARD OF EDITORS

HAROLD RUGG, *Chairman.*

J. CARLETON BELL — ARTHUR I. GATES — RUDOLF PINTNER — L. M. TERMAN
FRANK N. FREEMAN — V. A. C. HENMON — BEARDSLEY RUML — E. L. THORNDIKE

VOL. XIX
No. 6

SEPTEMBER, 1928

$5.00 a year.
70c per copy.

CONTENTS

NATURE AND NURTURE: THEIR INFLUENCE UPON INTELLIGENCE AND UPON ACHIEVEMENT. *Editorial Announcement* 361
THE INFLUENCE OF NATURE AND NURTURE UPON INTELLIGENCE SCORES. *Lewis M. Terman.* 362
AN EVALUATION OF THE EVIDENCE IN PART I OF THE YEARBOOK AND ITS BEARING ON THE INTERPRETATION OF INTELLIGENCE TESTS. *Frank N. Freeman* 374
OBSERVED FACTS AND THEORETICAL CONCEPTS. *Arthur I. Gates* 381
EDITORIAL IMPRESSION OF THE CONTRIBUTION TO KNOWLEDGE OF THE TWENTY-SEVENTH YEARBOOK. *Guy M. Whipple.* 389
INTELLIGENCE AS METHOD OF ADAPTATION. *Charles H. Judd.* . 397
HEREDITY AND ENVIRONMENT—OR CAPACITY AND TRAINING? *Bird T. Baldwin.* 405
TWINS AND ORPHANS. *A. H. Wingfield and Peter Sandiford.* . . 410
THE CONDITIONED REFLEX AND THE LAWS OF LEARNING. *George Humphrey.* 424
EYE PREFERENCE TENDENCIES. *A. M. and Marquerite A. Snyder* 431
NEW PUBLICATIONS IN EDUCATIONAL PSYCHOLOGY AND RELATED FIELDS OF EDUCATION. *Conducted by Frances M. Foster* . . 434

Published Monthly Except June to August by

Warwick & York, Inc.,

York, Pa. Baltimore, Md.

Entered as Second Class matter November 15, 1921, at the Post Office at York, Pennsylvania under the Act of March 3, 1879.

The Journal of Educational Psychology

is published monthly, by Warwick & York, Inc., Baltimore, Md., and York, Pa. Title page and index are bound in December number.

Manuscripts for publication, books, or other materials for review, and news items should be addressed to Harold Rugg, Chairman of the Editorial Board, 400 W. 118th Street, New York City.

The price of the Journal is $5.00 a year, payable in advance. Foreign postage is 30 cents extra. Single current issues are 70 cents each, and less than a full year's subscription is charged at single issue rate for every month ordered. Back volumes $5.50 each; single back issues 90 cents each.

Subscribers should notify the publishers of change in address at least four weeks before publication of the issue with which change is to take effect. No claim for non-receipt of an issue will be entertained unless made within two weeks after receipt of the next succeeding issue.

Warwick & York, Inc.,

Baltimore, Md.

TEACHERS' HEADQUARTERS

European

Plan

Entirely

Fire-Proof

Centrally

Located

EDWARD DAVIS

Manager

HOTEL RENNERT

BALTIMORE, MARYLAND

The Value of the Dictionary

is the title of a *new* booklet written to help you in Teaching the Dictionary. Here are a few suggestions of the lessons included:

First Dictionary Lessons
Relative Position of Letters
How to Find Words
What You Find
Pronunciation
How to Find Meanings
Parts of Speech and Meanings
Unusual Uses of Words
Synonyms
The Hyphen, Etc., Etc.

Copies of this new booklet will be sent FREE to teachers upon request.

G. & C. Merriam Company

Springfield, Mass.

Publishers of the Merriam-Webster Dictionaries for over 85 years

Look for the Circular Trade-Mark Get the Best

WEBSTER'S NEW INTERNATIONAL DICTIONARY

SCHOOL AND SOCIETY

A weekly journal covering the field of education in relation to the problems of American democracy.

The objects of SCHOOL AND SOCIETY are the advancement of education as a science and the adjustment of our lower and higher schools to the needs of modern life.

It encourages discussion of all problems from every point of view; it pays special attention to foreign affairs; it publishes accounts of educational events of the past week and research on fundamental problems.

Subscription $5.00—Single Numbers 15 Cents

SCHOOL AND SOCIETY will be sent to new subscribers for thirteen weeks for one dollar, which may be sent at the risk of the publisher.

THE SCIENCE PRESS

Grand Central Terminal - - New York, N. Y.

THE JOURNAL OF
EDUCATIONAL PSYCHOLOGY

Volume XIX | September, 1928 | Number 6

NATURE AND NURTURE: THEIR INFLUENCE UPON INTELLIGENCE AND UPON ACHIEVEMENT

SELECTED PAPERS READ AT THE BOSTON MEETING

OF THE

NATIONAL SOCIETY FOR THE STUDY OF EDUCATION,

FEBRUARY 25 AND 28, 1928

The editors of the Journal of Educational Psychology, in coöperation with the National Society for the Study of Education, print herewith seven[1] papers which formed the nucleus of the Society's recent meeting on the problem of nature *versus* nurture.

[1] Terman, Lewis M.: (1) "The Influence of Nature and Nurture upon Intelligence Scores: An Evaluation of the Evidence in Part I of the 1928 Yearbook of the National Society for the Study of Education." (2) "Replying to Criticisms of Part I, and Introducing Part II of the 1928 Yearbook of the National Society for the Study of Education." (3) Freeman, Frank N.: "An Evaluation of the Evidence in Part I of the Yearbook and Its Bearing on the Interpretation of Intelligence Tests." (4) Gates, Arthur I.: "Observed Facts and Theoretical Concepts." (5) Whipple, Guy M.: "Editorial Impression of the Contribution to Knowledge of The Twenty-seventh Yearbook." (6) Judd, Charles H.: "Intelligence as Method of Adaptation." (7) Baldwin, Bird T.: "Heredity and Environment—or Capacity and Training."

We remind the reader that it was impossible to secure written papers from several speakers at the Boston meeting and that therefore the remaining material is from that regularly submitted to the Journal of Educational Psychology.

THE INFLUENCE OF NATURE AND NURTURE UPON INTELLIGENCE SCORES: AN EVALUATION OF THE EVIDENCE IN PART I OF THE 1928 YEARBOOK OF THE NATIONAL SOCIETY FOR THE STUDY OF EDUCATION

LEWIS M. TERMAN

Stanford University

The issues involved in the nature-nurture controversy are of almost unequaled consequence for educational theory and practice. If the differences in intelligence scores which we find among children are due in the main to controllable factors of environment and training, then theoretically, at least, they can be wiped out by appropriate educational procedures—procedures which it would then become our duty to provide. If, on the other hand, they are due primarily to differences in original endowment, then the duty of the school is to provide differentiated types of training which will take these native differences into account.

Now as I have pointed out in my introductory chapter to the 1928 *Yearbook*, any proposal to apply in practice the doctrine of the biological inequality of human beings, whether in politics or education, infringes upon some of our deepest-lying prejudices, and challenges our fundamental philosophies of life. It was only natural, therefore, that all too frequently the controversy should have descended from the level of calm discussion of the scientific evidence to that of acrimonious asseveration. Catch phrases, like "educational determinism" or "democracy and the IQ" have become as charged with emotion as were once such slogans as "states rights," "abolition," or "taxation without representation." The belief in native differences and in the validity of intelligence tests is looked upon by some of the environmentalists as belonging in the same category as the theological doctrine of infant damnation. Unfortunately, the dogma of the environmentalists has too often been answered by the dogma of the hereditarians. Too many on both sides have shown more interest in finding and phrasing arguments to support a philosophical bias than in investigating the problem by the method of science.

Precisely because the problem *is* so controversial and has given rise to so much heated discussion, the committee charged with the preparation of the 1928 *Yearbook* has deemed it wise to make it a

research undertaking, instead of taking advantage of the opportunity to stage a combat of educational gladiators. Accordingly, the *Yearbook* has been devoted entirely to the dispassionate exposition of scientific investigations bearing on the nature-nurture problem; for the most part to *new* investigations.

It is obvious that the success of such an undertaking will depend largely upon the ability of those in charge to make an unbiased presentation. It was necessary to avoid any suspicion whatever of a desire to make the *Yearbook* the special mouthpiece of either side to the controversy. For this reason, a committee was selected which would represent all shades of opinion upon the issues involved, and the committee's sole concern has been to bring together all the evidence it was possible to secure on either side of the question, and to have the evidence presented as objectively as possible. I believe you will agree with me that without exception the contributors to the *Yearbook* have entered whole-heartedly into this spirit.

Now it is to be hoped that the readers of the *Yearbook* will profit from this example; that in appraising its contents they will divest themselves, insofar as is humanly possible, of their biases and predilections. How absurd, after all, is the attitude of mind which would force truth into any kind of preconceived mold, when only truth which is genuine and undistorted can give us the control over human nature and human destinies that our various institutions, including the school, are intended to exercise.

There is one other request which I think the committee has a right to make to the *Yearbook* readers: namely, that in appraising its work they should keep in mind the inherent difficulties and complexities of the problem, particularly the difficulty of securing factual data that are at once crucial and unambiguous. Those who do not bear this in mind are likely to be disappointed and discouraged by the lack of perfect consistency in the conclusions of various contributors. It would be unreasonable to expect from this *Yearbook* a final answer to the nature-nurture question. Certainly no such finality has been attained or even approximated. The most that can justly be claimed is that the bounds of our knowledge have been extended in some directions and in some measure.

Before entering upon a discussion of the new evidence that is before us, I wish to warn you against an unfortunate misconception that in the past has often entered to cloud the essential issue: namely, the idea that adherents of the "nature" theory assume a "glass bottle"

type of mental development which is quite independent of any stimulation from environment. No idea could be more misleading, and no phenomenon more impossible. The nature-nurture problem that faces us deals with human beings as they are—not as an experimenter might rear them in a "nurture-proof" laboratory. We are interested in the child as he comes to us—with his unique complex of ancestry, associates, home training, schooling, and physical and moral attributes. We are interested, not in finding out how he would have developed if he had had no environment at all, but in discovering whether and in what degree he can be made more intelligent by improving the conditions of his *milieu* within the limits found in reasonably good social communities.

With this in mind, let us consider, first, the influence upon intelligence scores of such factors as schooling, language handicap, physical conditions, and coaching on the tests, leaving until the last the consideration of the more ambitious and perhaps more crucial investigations of family resemblance.

In regard to the influence of schooling, I do not find any evidence in the *Yearbook* that the IQ is very greatly affected by the ordinary differences found among children with respect to school attendance. The evidence, so far as it goes, indicates that if this influence is present at all, it is relatively small. Miss Hildreth's forty-eight children who entered the first grade after having had nursery school training showed only a slight and perhaps not a statistically significant advantage in IQ over the forty-one in her control group who had not attended nursery school, and after a year and a half there was no evidence whatever of such an advantage. Miss Goodenough, in a similar study of two groups of twenty-eight pupils each, one group with and one without nursery school training, also failed to find any statistically significant difference. Those of Dr. Heilman's eight hundred ten-year-old subjects who had attended kindergarten for one year showed no advantage in IQ over those of his group who had not attended. However, Dr. Heilman did find a correlation of .3, and Miss Denworth (seven hundred subjects of the ages nine to twelve) a correlation of .2 between mental age and total days attendance since the first grade entrance, chronological age being held constant. These coefficients are of course impossible to interpret. They may mean (1) that the IQ is somewhat affected by school attendance; (2) that school attendance is somewhat influenced by brightness; or (3) that the influence has operated in both directions. Certainly it does not seem

unreasonable to believe that, everything else equal, dull children are more likely than bright children to be irregular in attendance.

As for the language handicap, which has been held to invalidate for so many subjects the results of intelligence tests, the *Yearbook* evidence suggests that it is probably not a major factor. Murdock and Maddow's rather careful comparison of test scores of children and the amount of English spoken in the home fails to show any significant correlation. This was true not only of scores in the Princeton non-verbal International test, but also of scores on the highly verbal Otis Examination. Peterson's non-verbal Rational Learning test, applied to white and negro children, yielded about as large differences between the two groups as we ordinarily get when the Binet test is used. Peterson's negro children of ten scored appreciably lower than white children of eight. This would suggest an average IQ something less than eighty for negro children, a figure which agrees rather closely with Binet results. The mere fact that school children of certain nationality groups ordinarily test low and also come from homes where only a limited amount of English is spoken, in itself proves nothing. The fact is, there are other nationality groups of school children similarly situated, who test high. Perhaps many of you have wondered, as I often have, why this supposed language handicap should operate when the home language is Italian or Spanish and not operate when it is German, or Scandanavian, or Yiddish.

With regard to coaching on the subject-matter of the tests, the evidence is fairly unambiguous. The studies agree in showing that even a few hours of direct coaching may completely invalidate test results. Indirect coaching, that is coaching upon material similar to but not identical with that found in the tests, is effective to a much smaller degree. Fortunately, the results of the different investigators further agree in showing that the effects of coaching, whether direct or indirect, are relatively short lived. The study of Gates, which seems to me particularly conclusive on this point, shows that extended coaching on the test of memory span for digits, although bringing a large temporary improvement, has completely lost its effect within a few months, both with identical and similar material.

The question as to the effects of coaching is obviously related to the question of formal discipline, or spread of training. The attempt to bring about a permanent increase of intelligence, or power to think, by the method of formal discipline, is seen to be one form of what we have called indirect coaching. On this much debated question I leave you

to draw your own conclusions from Professor Whipple's fair-minded summary in Part II of the *Yearbook*. I agree with Whipple that the results to date from investigations of formal discipline call for suspended judgment. At the same time I am inclined to believe that about the hardest thing in the world to improve is the ability to think. Judd is doubtless justified in urging that we, as teachers, should do everything in our power to foster the habit of generalizing and searching for meanings. I am inclined to think that on the given child's level of mental ability something may thus be effected. Whether the child's mind can thus be brought to function permanently upon a higher mental level, I do not know. I think we are justified in examining very closely all alleged "jacking-up" effects in the realm of intellect.

On physical conditions as determiners of the IQ, the *Yearbook* furnishes evidence of two kinds. By a careful followup of three hundred forty-three children over a period of two years, Miss Hoefer and Miss Hardy have tried to find out whether improved physical conditions tend to be correlated with improvement in intelligence and school achievement. The authors seem to feel that their data rather point to the existence of such a correlation, though they admit that if it exists at all it is very small. From an examination of the data they present I do not feel that their correlations can be regarded as statistically convincing. This is in line with the earlier negative results on the supposed influence of adenoids and diseased tonsils on intellectual ability.

It is unfortunate that we have not been able to secure for the *Yearbook* any studies on the supposed influence of nutritive factors. However, interesting data of another kind have been presented: namely, on the effects of puberty præcox. Some have held that the curve of a child's mental growth is perhaps considerably correlated with the physical changes that accompany sex maturation, although no real evidence in support of this theory has ever been offered, and the data of Gates and others contradict it. The summary of literature by Stone and Doe, and the new data by Gesell, appear to be conclusive. Intellectual ability is not influenced materially by early sex maturity.

Let us now turn to the investigations on family resemblance. Sib resemblance on the I. E. R. test is found by Thorndike to be represented by a correlation of .60. That this correlation is some eight points higher than Pearson has established for physical traits may or may not mean that the total influence of home environment is repre-

sented by the difference between .52 and .60. I doubt whether Thorndike's suggestion to this effect was intended to be taken very seriously. Sib resemblance alone, of course, proves nothing. It might all be due to nature, all to nurture, or part of it to nature and part to nurture. Tallman's data on twins, which show that the average IQ difference between twins of one-egg origin is only about sixty per cent as great as that between twins of two-egg origin, are more conclusive; also the data by Burlingame and Stone which show a sib resemblance of .31 in the intelligence scores of white rats, even in a rather inbred colony. (The effect of inbreeding would presumably be to lower the correlation.) We can at least be fairly certain that this sib correlation of .31 was not due to cultural influences in the rat families!

As regards parent-child resemblances, Jones and Burks find a correlation in the neighborhood of .55 between parents and offspring living in the same home. Willoughby's somewhat lower figures are, for reasons unknown to me, out of line with the results of other investigations. However, even if we assume a correlation of .5 or .6, no conclusion whatever is justified. It might be due entirely to environment, entirely to heredity, or partly to each. The Chicago and Stanford investigations are the only ones that have ever made a serious attempt to disentangle the two factors.

The Stanford investigation yields a correlation of .56 between parents and their offspring living together. In the case of foster parents and their foster children adopted in the first few months of life, the correlation is .09 in the case of foster fathers and .23 in the case of foster mothers. There is the bare possibility that even this small correlation may be due wholly or in part to selective placement of the children, to the conscious or unconscious attempt of placement agencies to put an "A" child in an "A" home, a "B" child in a "B" home, etc. It should be emphasized, however, that Miss Burks made every effort to eliminate the influence of selective placement. For this reason she ruled out the race and nationality factors. Partly for this reason, also, she included only foster children who were adopted in babyhood, when "A" babies are hard to distinguish from "B" or "C" babies. So carefully were extraneous factors controlled in this study that I regard Miss Burks' data about as crucial as it is humanly possible to secure, number of cases considered. Two lines of statistical treatment of the data yield convergent results. By one method it appears that home environment accounts for about seventeen per cent

of the variance of children in intelligence. By the other it appears that a home environment one SD above or below the average may raise or depress a child's IQ by six or eight points. I do not see how Miss Burks could possibly have drawn any other conclusion from her data than those which she has drawn.

Whether fortunately or unfortunately, the Chicago investigation does not wholly support these conclusions. It finds that in one hundred thirty sib pairs, one member of a pair having been placed in a superior home and the other in an inferior home, those placed in superior homes average nine points higher in IQ than those placed in inferior homes. This is a somewhat greater difference than Miss Burks' data would have led us to expect, especially in view of the fact that the Chicago sib pairs had not been separated their entire lives, but on an average only about seven years.

Other data from the Chicago study show less agreement with those of the Stanford study. The foster parent-foster child correlations are far higher than Miss Burks found. Parents and their own offspring who have been separated several years yield a much lower coefficient than is usually found when they have not been separated. Foster children living in the same home with "own" children for several years are found to resemble the latter even though they are not related. The correlations of .25 to .34 may or may not be significant in view of the rather large probable errors involved.

The numerous lines of evidence presented in the Chicago study are for the most part consistent with each other in suggesting a considerable influence of environmental factors. I hope I do not need to affirm my high regard for the scientific ability of the authors or my admiration for their careful work and their objective exposition of results. At the same time I must say that I do not find their data conclusive. I am not convinced that they have ruled out the spurious correlation that would arise from selective placement. Their foster children were adopted at an age when selective placing would conceivably have been possible. That it did operate to some extent is indicated by the correlation of .34 of foster parents and foster children *at the time of adoption.* It is true that several years after adoption the correlation was .52, but as the probable errors were .07 and .06, respectively, one can not be absolutely certain that a real increase in resemblance has taken place. If the home influence were very important one would expect to find a clear correlation between IQ of child and time spent in foster home, but only the merest suggestion of such

a correlation was found. The correlations of .25 to .35 between unrelated pairs in the same home might, conceivably, have been caused wholly or in part by selective placement. The fact that foster children placed in good homes averaged nine points higher than their sibs placed in poor homes, might also have been due to selective placement; that is, to a tendency among the more intelligent foster parent to pick the more promising of two sibs for adoption. I do not assert that selective placement could reasonably be held to account for *all* of the differences between the conclusions from the Chicago and Stanford studies. It may possibly account for the greater part of them. It may possibly account for the fact that the Chicago study is the one which, of the many contributions in the 1928 *Yearbook*, stands most nearly alone with respect to the influence it finds for environmental factors.

REPLYING TO CRITICISMS OF PART I, AND INTRODUCING PART II OF THE 1928 YEARBOOK OF THE NATIONAL SOCIETY FOR THE STUDY OF EDUCATION

Two of the speakers on the Saturday evening program of the Society expressed themselves with considerable feeling in regard to the conclusion voiced by some of the *Yearbook* authors on the stability of the IQ and the importance of mental age as a determiner of achievement. They asserted that the acceptance of the doctrine that intelligence test scores are not readily influenced by schooling amounts to a rejection of the principle of mass education and implies that we might as well discard our alphabet, nail up our schools, and retreat to the jungle. This is like arguing that, unless universal musical training enables us to make all children test close to the upper limit of scores in the Seashore Music Test, we might as well abandon our modern system of music notation and exchange the piano for the tom-tom.

I am sure that an audience of the intelligence level of this one will not be misled by so obvious an attempt to sidetrack the issue. You will understand, I am sure, that the question is not whether mass education is a necessary condition of social and intellectual progress. No one today doubts that it is. Everyone admits that but for our schools we should be today like our ancestors of three thousand years ago—illiterate and uncultivated barbarians. I must confess that I am amazed, and also somewhat depressed, that such arguments

should be offered with all appearances of seriousness and with so much emotion—but perhaps it is the emotion that accounts for their being offered! Certainly nothing could be more unjust than the charge that belief in the substantial validity of intelligence tests stamps one as an enemy of public education.

The fact is, of course, that the introduction of the alphabet, and the resulting developments in increased demands upon abstract thinking, have lifted the IQ to a position of importance it did not previously possess. Among primitive men no very great intelligence was necessary to place an individual in possession of all the social inheritance that was open to him. Today it is far different. The total social heritage is so huge that no one, however intelligent, can take over all of it. The question is whether the amount a given individual can take, under a given pressure of education, does not depend largely upon his native ability. At any rate it is clear that the more important our social heritage is, the more important it is to give to each child his maximum opportunity to come into as full possession of it as his individual capacity will permit. That opportunity is now denied to the large majority of children who are considerably above or below the average child in intelligence. It is perhaps significant that the one contribution of Part II which presents evidence of marked influence of a special kind of school procedure in producing variations in achievement, is that by McCall which indicates that children of high IQ who have been segregated in special classes make about eight months more gain in achievement in a period of two years than do children equally gifted taught in the regular classes.

Accordingly, my interpretation of the evidence in Part I is that it places the IQ and other indices of intelligence on a solider foundation than ever before. Everything goes to show that the IQ does count, that it is not easily influenced by environmental factors, and that it is therefore relatively constant. I think I am justified in repeating what I wrote a dozen years ago; namely, that notwithstanding minor fluctuations in apparent ability with increasing age, generally speaking the feeble-minded remain feeble-minded, the dull remain dull, the average remain average, and the superior remain superior.

A few decades ago there were environmentalists who believed it possible to wipe out the whole range of IQ differences among unselected children. At present this group has dwindled to one or two of the more stubborn theorists. The contention of the environmentalists is that the IQ is not *absolutely* constant, that it can be influenced by

schooling or other environmental factors to a greater or less degree. Even the so-called hereditarian need hardly quarrel with this way of putting it. Personally I have never doubted that the IQ could be influenced to some extent. In expressing my views on the question of IQ constancy I have always been careful to say that the IQ is *relatively* constant, not that it is entirely beyond the influence of environmental factors.

Now I would not belittle the importance of small changes in the IQ. If I had a child of seventy IQ there is hardly any expenditure of money or effort that I would not regard as worthwhile if, thereby, the child could be made to respond in all respects like a typical child of eighty-five IQ, for the difference between seventy IQ and eighty-five IQ is very real and very significant. Indeed, one might take the view that the harder the IQ is to change, the more justified we are in making great expenditures of time and effort to produce even moderate improvement, just as in the economic world, goods which are laborious and difficult to manufacture command a higher price than goods which may be cheaply produced.

With respect to the whole nature-nurture problem, however, one must admit that there is relatively little which we can set down as precisely established fact. Whichever way we lean our conclusions must still be tentative if we are not to run the risk of having them overthrown by the results of later investigations. On one thing, however, I think we should be able to agree; namely, that we can never settle the nature-nurture controversy by oratory and the forensic tactics of the college debater. The primary purpose of oratory is to stir the emotions, not to enlighten the intellect. Educational evangelism may be all right in its place but it is a poor substitute for science in the search for truth.

Let us turn now to a consideration of the contents of Part II of the Society's 1928 *Yearbook*.

I understand that some of you are much concerned about the conclusions set forth regarding the relatively small influence exerted by variations in pupil attendance, teacher merit, and school costs in accounting for variations in the achievement of school children. I will admit that at first blush the results are surprising and perhaps disturbing. For example, Heilman and Denworth agree from their studies of large groups of children that probably not more than ten per cent of the variations in achievement can be accounted for by the variation in number of days of school attendance; also that achieve-

ment is determined chiefly by mental age. O'Brien's data on the efficacy of the seven month and eight month school term are in line with those of Heilman and Denworth. It is of course possible that more searching investigation may justify us in attributing a larger influence to the attendance factor, but in my opinion this is unlikely. I do not believe that attendance deficiency, amounting even to twenty or twenty-five per cent, will be found to compare with level of mental development as a determiner of school achievement.

Still more disturbing are the results by Holy and Ruch, showing for a large number of cities in Iowa a total lack of correlation between school costs and the achievement of pupils at the end of the twelfth grade. One cannot help but question whether the data secured can be taken as representative. Naturally all of us hope that further investigation will warrant a different conclusion. In any case, however, we must not get frightened and stop our investigations. Whatever the facts are they must be brought to light. If we who are friends of education do not get the facts and make right use of them, others less friendly to the public schools will get them and make wrong use of them.

The investigation of Taylor on the influence of teacher merit on pupil achievement is a little more hopeful. He found at least a suggestion of a positive correlation between teacher merit and pupil achievement. Although at best the correlation seems to be very low when the results of only a single term are considered, it is possible that this influence accumulating constantly during the entire school period would be of considerable importance.

On the whole I think we must admit that, although Part II is far from conclusive, the evidence suggests that subject mastery is less easily influenced by ordinary differences in school attendance, by ordinary differences in school expenditures, and by ordinary differences in teacher merit than most of us have assumed; also that it is more influenced by mental level than many have assumed. Apart from the experimental evidence of this *Yearbook* there is a rapidly accumulating body of experience that points to this conclusion. For example, in any school grade we find that what a given pupil can do depends far more upon the general intellectual level he has attained, as shown by a mental age score, than it depends upon the amount of time he has devoted to the study of the various school subjects. It has been demonstrated that even in a university of high standards it is possible for an exceptionally intelligent student to enter and make good marks without any previous high school training.

Suppose it should later be proved as it, of course, has not yet been, that a child can attain about the full arithmetical altitude that his mental level permits with fewer hours of instruction than the average child of his age is supposed to have had. Did not Rice prove two decades ago that many school systems which were giving twice the usual allotment of time to arithmetic were not producing any better results in that subject than other schools giving the average amount of time? Perhaps we can liken the developing child to a sponge—a growing sponge that is gradually increasing its capacity for absorption. At any given time the sponge will absorb only a certain amount of water even if you cast it into the ocean. In the case of the child, absorptive capacity seems largely determined by general mental level.

I see no reason whatever to take alarm even if it could be shown without any doubt that a child can be as well prepared for high school, so far as subject mastery is concerned, on three or four hours of daily instruction and eighty per cent regularity of attendance. Would it follow that our schools should therefore be reduced to three hour daily sessions and the length of term decreased by twenty or thirty per cent? The answer is that life is more than reading, writing, spelling, history, arithmetic, and geography, and that the school also ought to be more than these.

My own interpretation of the *Yearbook* evidence is that it suggests the desirability of reconsidering our educational aims. Possibly we have been laying too much stress upon the mastery of the school subjects. It is possible that we have over-stressed both the efficacy and the importance of educational methods and devices designed to boost achievement. I shall not be surprised if we are in time compelled to modify our educational ideals in several respects. In the first place, I think we shall ultimately come to place more emphasis than we now do upon the ethical and social ends of education, and care more than we now do about making the school a wholesome place to live. In the second place, I think that we shall in the future stress more than we now do the training of the child in attitudes and interests, as contrasted with the imparting of subject mastery. In the third place, I feel reasonably sure that in the future, instead of abandoning the measurement of general intelligence and special abilities, we shall make greater and greater use of these and kindred instruments.

AN EVALUATION OF THE EVIDENCE IN PART I OF THE YEARBOOK AND ITS BEARING ON THE INTERPRETATION OF INTELLIGENCE TESTS

FRANK N. FREEMAN

University of Chicago

I. The Question to Be Discussed

The question which I shall discuss is as follows: Does the evidence presented in the *Yearbook* concerning the influence of environment upon intelligence throw any new light upon the factors in intelligence and upon the nature of the ability which is measured by intelligence tests? Does the evidence indicate whether intelligence tests measure a fixed innate power or whether they measure only performance? Is the performance which is measured determined in part by experience and training as well as by the innate structure of the brain? If the performance which is measured by intelligence tests is determined in part by experience and training, is the part which these factors play negligible or is it important?

The question is broader than the mere question of the interpretation of intelligence tests. The scores on intelligence tests constitute the best evidence we have concerning the intelligence of human beings. The theories of the innate character of intelligence are based chiefly upon these scores. If we find, therefore, that they are determined to an important degree by environment, we may legitimately conclude that intelligence itself is determined largely by environment. If one were to hold that there exists an intelligence which is innate but which cannot be measured by an intelligence test, such an opinion would be purely a matter of speculation. Our present theories have been based upon the interpretation of our present tests and our task is now to interpret those tests.

II. The Evidence Should Be Examined without Biological Bias

Much of the discussion concerning the interpretation of intelligence tests and the nature of intelligence has been influenced directly or indirectly by certain biological theories of heredity. The Mendelian Law of inheritance has been applied in such a way as to make it seem almost certain that intelligence is inherited as a unit character. When this idea has once found lodgment in one's mind, it is very

difficult to give it up or to examine the factual evidence in an impartial way. It is well to know, therefore, that the earlier crude forms of this theory as they apply to human characteristics, such as intelligence, are now superseded by more flexible theories in the thinking of biologists themselves.

As a groundwork for our thinking upon this question, I shall present three quotations from the writings of Professor H. S. Jennings.[1]

"It is not true that particular characteristics are in any sense represented or condensed or contained in particular unit genes. Neither eye-color nor tallness nor feeblemindedness nor any other characteristic is a unit character in any such sense. There is indeed no such thing as a "unit character" and it would be a step in advance if that expression should disappear (page 18).

"It is not true that a given set of genes must produce just one set of characters and no other. It is not true that because an individual inherits the basis for a set of characteristics, he must have those characteristics. In other words, it is not necessary to have a certain characteristic merely because one 'inherits' it. It is not true that what an organism shall become is determined and foreordained when he gets his supply of chemicals or genes in the germ cells, as the popular writers on eugenics would have us believe. The same set of genes may produce many different results depending upon the conditions under which it operates. True it is that there are limits to this, that from one set of genes under a given environment there may come a result that no environment can produce from another set, but this is a matter of limitation, not of fixed and final determination; it leaves open many alternative paths (page 40).

"Or, more accurately, men and other organisms do not inherit their characteristics at all. What their parents leave them are certain packets of chemicals which under one set of conditions produce one set of characters, and under other conditions produce other sets. In man, the number of diverse sets that may thus be produced is very great; although it is of course not unlimited, but what the limitations are cannot be stated from general biological principles or from what we know of any other organism; they can be discovered only by concrete studies of man himself (page 55)."

It is the purpose of this paper to report the results of such a concrete study.

---

[1] Jennings, H. S.: "Prometheus."

III. The Evidence from Studies in the Yearbook

I shall present as briefly as I can the most important results recorded in the *Yearbook*. Because of my especial familiarity with the Chicago study, I shall mention the results of that study first and then compare them with the other studies.

(*A*) *The Gain Made by a Group under Improved Conditions.*—The best evidence of the effect of environment or any other factor is the measurement of the change in intelligence in the same group of children after a change in the environmental factors have been in operation for a specified length of time. In the Chicago study, we made such a "before and after" comparison with a group of seventy-four children. These children had been placed in foster homes at the average age of eight years and had remained there for an average period of four years. Their average IQ at the time of adoption was ninety-five. The average IQ after four years, when we made allowance for the fact that the standard score gives a drop of five points between the ages of eight and twelve, was one-hundred two and a half, a gain of seven and one-half points. When we divided the children into two groups according to the grade of home they were in, we found that the half who were in superior homes had made a gain of over ten points and had a final average IQ of one hundred five. Those in the poorer homes had gained but half as much, or five points.

The gain of ten points in IQ at the middle of the distribution is extremely significant. It represents an advancement past one-third of children in general. At the beginning of the period, the children were at the bottom of the middle third, and at the end of the period they were at the top of the middle third.

The other studies reported in the *Yearbook* in which the measurement of children's IQ before and after a given change has been made deal with younger children and with smaller numbers through a shorter period of time. Miss Rogers and her colleagues found that children placed in an institution showed no gain after one year. Miss Hildreth found that the pupils in the Lincoln School who had not had previous schooling made six points more gain in IQ than did the pupils who had had previous schooling. Miss Goodenough found that twenty-eight nursery school children made about three points more gain in IQ than did a parallel group of children who were not in school. Mrs. Woolley reports an average superiority of nursery school children of eight points in IQ.

The results of these studies, then, are in general positive. They indicate that improved environment may raise the child's IQ as much as ten points. The studies are not entirely consistent, however, and further studies need to be made.

(*B*) *Resemblance of Siblings Who Have Been Separated.*—It is generally known that brothers and sisters, or siblings, resemble each other more than do children in general. The studies of Pearson indicate that the resemblance in physical traits is represented by a correlation of about .50. The study by Thorndike reported in the Yearbook indicates that the intellectual resemblance of siblings of the high school age is about .60. This resemblance is commonly supposed to be due to the fact that siblings have a common inheritance, but it is obvious that it might be due in part, at least, to the fact that they have a similar environment.

The Chicago study indicates that the resemblance of siblings in intelligence is due in part to similar environment. The evidence for this conclusion lies in the fact that siblings who have been separated and brought up in different homes resemble each other less than the siblings in general. The correlation between the intelligence quotients of our entire group of siblings was considerably lower than .50. There are two methods of calculating the correlation in such a case as this, one of which may be called the "age entry" and the other the "double entry." By the first method the correlation was .34, and by the second, .25.

If the similarity of home environment is responsible in part for the similarity in the intelligence of siblings, we should expect those who are separated early to be less alike than those who were separated late, and this we find to be the case. Furthermore, we should expect, on this assumption, that those who have been separated for a long period of time would be less alike than those separated for a short period of time. This also we find to be true. Finally, we should expect those who are placed in similar homes to be more alike than those who are placed in dissimilar homes. This we find to be true.

In order to trace more specifically the effect of the home upon the intelligence of siblings, we divided the group into two equal smaller groups putting into one group the child of each pair of siblings who was in the better foster home and in the other group the one in the poorer foster home. The average IQ of the siblings in the poorer homes was 85.7, and of those in the better homes, 95, a difference of 9.3 points.

No other study in the Yearbook corresponds to this. Our findings, however, may be compared with those of Tallman, who measured the amount of difference between siblings in general, unlike twins, and like twins. Like twins or identical twins are, of course, more alike intellectually; and unlike twins are somewhat less alike; and siblings in general still less alike. The significant fact, however, is that unlike twins resemble each other almost as much as do like twins, whereas siblings resemble each other much less. The difference in the resemblance between like and unlike twins is 2.85, and the difference between unlike twins and siblings is 4.03. Unlike twins and siblings should resemble each other almost equally if heredity were the only determining factor. Environment, therefore, must be responsible for the fact that siblings resemble each other so much less than do unlike twins and that the difference between the degree of resemblance of unlike twins and identical twins is so little.

(*C*) *Resemblance of Unrelated Children in the Same Family.*—If living in the same home contributes to the resemblance of siblings, it may be expected to produce a resemblance between two unrelated foster children or a foster child and an own child in the same foster home. We find, in fact, that there is such a resemblance. The correlation between the IQ's of pairs of unrelated foster children in the same home is .37 and the correlation between the IQ's of pairs of foster children and own children in the same home is .34. Thus, unrelated children in the same home resemble each other fully as much as do brothers and sisters who are in different homes.

(*D*) *The Factor of Selection.*—It is conceivable that the resemblance between unrelated children in the same home, and other resemblances to be mentioned, are due not to the influence of the home but to the exercise of a discrimination among children in selecting them for the homes in which they were placed. In both the Stanford and the Chicago studies, a good deal of study was given to this question. The methods by which children are placed and the circumstances of adoption were examined. From this examination it appears that neither the agents who placed the children nor the foster parents appeared to take the intelligence of the child into account in deciding upon its adoption. Statistical evidence, also, indicates that the intelligence of the child has very little influence on his placement. For example, there is as close a correlation between the child's IQ and the rating of the foster home in a group of children concerning whose parents practically nothing was known and who were placed at the average age of two years, four months, as in the case of all the children.

(*E*) *General Level of Intelligence of Foster Children in Comparison with That of Their Parents.*—The Stanford and Chicago studies agree also in finding that the average IQ of the foster children is above the probable IQ of their parents. The average IQ of the Stanford group of children is 107.4; that of the Chicago group is 97.5. The lower average of the Chicago group is due to the fact that they were adopted later, that they were older at the time of the test and probably to the fact that a larger proportion of them are legitimate and in homes which average lower than those of the Stanford group.

How superior we shall judge the IQ's of these children to be to those of the parents depends, of course, on our estimate of the parents' intelligence. Miss Burks makes the rather surprising estimate that if they were tested, their IQ would be found to be slightly above the average. This estimate is based on rather meager data concerning the occupations of the true fathers and mothers. Concerning the mothers, over eighty per cent of whom were unmarried mothers, the estimate is particularly doubtful in view of the fact that the largest single group are housemaids. One of the most uniform findings of mental tests is that mothers of illegitimate children are in general of low grade intelligence. Only strong positive evidence could counter-vail the weight of these findings.

In the case of the Chicago group there is more direct light thrown on the mentality of the true parents. The occupational level of the fathers is low. This is especially true of the fathers of the legitimate children. Of the unmarried mothers of whom there is record, half are maids, waitresses or factory workers. On about seventy-five per cent of all the parents except the fathers of the illegitimate children, we have an estimate of intelligence. One-quarter of the mothers of the illegitimate children and of the fathers of the legitimate children, and one-half of the mothers of the legitimate children are reported to be mentally defective. This certainly does not present the picture of an average group of parents. A reasonable estimate would seem to place the mid-parental intelligence of the parents of the legitimate children no higher than the upper eighties, and of the illegitimate children as not above ninety-five. Such estimates would indicate that the children's IQ is at least ten points above that of their parents. This is a conservative estimate.

It is instructive to compare the average IQ of the foster children in the Stanford group with the average IQ of a parallel group of own children in homes of the same grade. The averages are 107.4 and 115.4 respectively. I have estimated that the homes were responsible

for raising the average of the foster group ten points or more. The superior heredity of the own children was evidently responsible for an additional superiority of eight points more. This may perhaps justify an estimate that the environmental difference under consideration has as much or more effect than the parental difference in heredity.

(*F*) *Correlation between the IQ's of Foster Children and the Grade of the Foster Home or the IQ's of the Foster Parents.*—If the foster home influences the child's intelligence, we should expect the superior homes to produce a greater improvement than those which offer lesser advantages. We have already seen that this is true when siblings are placed in different homes. We have also divided the entire group of four hundred one children into three sub-groups according to the grade of their foster homes. The average IQ of those in the lower group of homes is 88.9 and of those in the upper group 106.8, a difference of eighteen points. The correlation between the IQ's of the entire group and the rating of their foster homes is .48.

In the Stanford study the corrected multiple correlation between several features of the environment and the foster children's IQ's was only slightly less, namely, .42. This indicates a rather marked relationship. The two studies, therefore, agree in finding a substantial relationship between the character of the home and the intelligence of the children. These findings furnish good evidence of the influence of the home on the child's intellectual development.

These correlations may be compared with the correlation between the IQ's of children and own parents. The studies of Jones and Burks in the Yearbook agree in finding this to be about .60. This correlation is the joint result of the effect of heredity and of environment. We may say, then, that the influence of environment alone gives a correlation of about .45, while the addition of the influence of parental heredity raises the correlation to .60.

IV. Conclusion from the Studies

The preponderance of evidence appears to me to indicate beyond a doubt that the scores on intelligence tests—and hence intelligence, so far as we can now measure it—are influenced to a marked degree by the character of the home and the other circumstances which go with it. While we cannot make any exact comparison between the relative amount of influence of heredity and environment, we are justified in saying that environment is not a minor factor. It is clear that environment be taken into account both in interpreting the scores on intelligence tests and in estimating the importance of education.

OBSERVED FACTS AND THEORETICAL CONCEPTS

ARTHUR I. GATES

Teachers College, Columbia University

My discussion will be concerned, chiefly, with what I consider to be misconceptions arrived at by various persons who have read the two *Yearbooks*. In presenting misinterpretations, I shall find it necessary also to offer the true conceptions as I see them.

Misinterpretations of data in the two *Yearbooks* are due in some degree, I think, to the fact that different meanings are attached by different authors to such terms as intelligence, IQ, acquired ability and the like. Dr. Freeman, for example, uses the term intelligence to mean merely the score of the Binet test, and not something which lies behind this score. Other authors mean by intelligence various things which lie behind the test. Thus Dr. Heilman writes: "In this study acquired abilities are represented by the Stanford Achievement tests . . . and inborn capacity by the Stanford-Binet Scale." Both of these writers use their arbitrary definitions consistently and therefore properly. But the shift in meaning from article to article results in misconception by the unwary reader.

The last statement quoted, for example, is read by many to mean that the Intelligence Test does not measure acquired ability and that Educational Tests do not measure native intellectual capacity.

Performance in the Stanford-Binet test, of course, is not instinctive. Abilities to do the exercises in this scale do not grow in some mysterious fashion. They must be learned. Nothing does a child more truly learn than to name colors, identify coins, comprehend English words, count and do other exercises in this test. The Binet is no less dependent upon learning than any other test.

Recognition of this fact will, I think, help to clarify the findings in both volumes. The problems studied in Volume II are not to be sharply distinguished from those dealt with in Volume I. Both are concerned with the influence of various factors upon achievement.

We may raise a few general questions and seek their answers in the *Yearbooks*.

The first question is: *Will special or intensive or exceptional training given to any children increase the score on the Binet and other tests?* Dr. Greene's study shows that specific instruction for only two hours will increase the score on the Binet test from twenty to thirty per cent.

She demonstrates, furthermore, that training on similar but not identical functions for two hours will increase the score ten to twenty per cent. Dr. Freeman and Dr. Burks have shown that in the best homes, more things are learned which influence score on this test than are learned in the poorest homes, although in four years time the educative influences of the best families upon scores in the Binet test exceed those of the poor families by five to ten per cent. It is shown in the first volume, then, that certain educative advantages over the average do increase scores on the Binet test appreciably just as it is shown in the second volume that advantages in the length of school training and in other respects increase at least slightly the scores on achievement tests.

The next problem is: *What do these increases in scores, especially those on the Binet test, imply?* Do they mean anything more than that you can teach children to do things which they can do only after they have learned to do them? Do they mean that constitutional factors—capacities—are also increased correspondingly? It is when this question comes up that readers become confused by the changing use of terms and their own preconceptions. Many readers, I have found, conclude that the increases of from ten to thirty points IQ obtained by Dr. Greene in a couple of hours represent merely an increase in acquired ability to do the test and not a gain in *real* intelligence, but that the gain of from five to ten per cent brought about by the superior homes during a period of four years is a gain in real intelligence.

Now, unless evidence to the contrary is brought forward, the rules of science and logic both require that we interpret both results in the same way. Dr. Greene gives some evidence that after the special education is stopped the gain in IQ is not permanent. This regression may be explained partly, perhaps, as a result of loss rather through disuse and partly because as the pupil's age advances, the exercises on the test which they were taught to do fall below the crucial level. In Dr. Freeman's study and most of the others there is no evidence on this point. To secure evidence, the children would have to be deprived for a considerable time of those educative factors which distinguish the good from the poor home. In sum: *A gain in score on the Binet test means merely, unless other evidence to the contrary is presented, that children have learned directly or incidentally to do certain exercises on the test and nothing more.* There is little evidence in any of these studies that it is anything more.

If this is the case, we must ask: *Why did any test, in particular, why did the Binet test come to be called a test of intelligence?* How can a test of acquired abilities measure native intellectual capacity?

The answer is, that hundreds of studies during the last thirty years have tended to justify the theory that when, for a given group of individuals, all educative factors are the same, wide individual differences in ability still remain. If educative influences for a group of children are identical and individual differences still appear, these variations must be explained—as due to mysteries, spirits, demons or something else. The conception to which scientific discoveries point is not to demons but to native constitutional factors or capacities. The theoretical conception is precisely this: *When educative forces for a number of persons of the same age are absolutely identical, individual variations in ability appear which, under these conditions, may be attributed to differences in native constitutional factors.*

The next question is: *Are educative influences in a typical American community ever near enough to equality to make it possible practically to gauge capacity by measuring achievement?* The answer, which I believe to be fairly well justified by the studies in the *Yearbook* and elsewhere reported is: Yes.

I should like to indicate a few circumstances which justify this answer.

In the first place, it should be noted that the *Yearbook* shows that advantages in home influences as well as in school factors influence scores on various tests. It should also be observed that the influences of the home and school tend to have an opposing effect upon individual differences. The studies of Thorndike, Burke, Freeman and others show that good homes increase the scores more than poor homes. Since it is in general true that the better the home is, the better the native constitution of the children in them, the effects of nature and nurture combine to increase individual differences on the test scores. The better the child the more effective his home training. The influence of the school is exactly opposite, as Kelly has recently shown. The poorer the native endowment of the pupil the more vigorously and effectively he is educated; the better the child the less vigorous and effective the treatment in school. The school, then, exerts pressure to reduce individual differences whereas the home operates to increase it. These two opposing forces tend to neutralize each other with the result that, for all save a conspicuous few children of the same age, the educative influences upon achievement in the Binet test

are substantially equal. *The Binet test works practically as well as it does, not because it measures different kinds of abilities, or different mental factors from other tests, not because it is not subject to the influence of achievement, but merely because it measures what is learned in about equal degree in the school and in the out-of-school environment.*

To realize the fact that achievement is the result of many educative factors, which affect individual variations in different, sometimes compensating ways, will help us to avoid certain misinterpretations of the data in Volume II.

Consider for example the following quotation from this volume.

"Regardless of shortage in the educative process, even of a year or two, children tend to attain a proficiency in school subjects close to that appropriate to their mental level. We are reminded of Breed's experiment in which young chicks that are prevented from learning to peck grain during the first week or two become, when released, as proficient almost at once as control broods which had practiced pecking from the time of hatching." Certain readers of this *Yearbook* have said that this sort of statement gives the following implications:

(1) Education is largely futile—reading and arithmetic develop from inner growth and exposure like the pecking of chicks.

(2) It matters little how much or in what manner you teach—school achievement will only reach the mental level anyhow.

(3) Efforts to improve the technique of teaching and learning are likely to be largely futile—mentality determines the rate and limit of achievement.

Such implications are, of course, quite unjustified. They spring, I think, from a confusion of the idea of individual variations in achievement under condition of near equality in nurture with the idea of the absolute amount of achievement made by anyone or everyone. The mere fact that individuals differ in achievement when educative factors are nearly equal does not imply anything about the absolute effectiveness of education—whether it is good, mediocre or bad.

The idea of achievement necessarily being limited to the mental age level is due to the erroneous notion that the mental test and educational test directly measure very different things and the equally erroneous notion that achievement on the Binet tests for the average ten year old will always be what it is now. The Binet test, as I have said before, nearly measures what children have learned to do under present conditions. Although no one now has any evidence either for or against the possibility, it may be that the average achievement of

twelve year old children today on these tests will, twenty years hence, be equalled by the average child of ten. Finally, as Kelly has recently declared, the Binet Test may measure in very large degree the same abilities as a group of tests in the fundamental school subjects. If this is the case, there is no need to be surprised that children measured in both types of tests should approximate the same levels. If this is true, furthermore, we may expect that in the future improvements in education will raise scores in intelligence tests substantially the same as in educational tests.

Many readers have expressed surprise that Volume I should show that educative factors influence "intelligence" so much whereas Volume II indicates that other educative factors should affect "achievement" in the school subjects so little. For reasons already given the fact that educative factors influence mental tests score considerably need occasion no surprise. It has been suggested furthermore that it is misleading—unless other evidence is presented—to call a gain or loss in such scores a gain or loss in constitutional factors. I should now like to suggest that the findings in Volume II are no more surprising and that they are—although in a different way—equally subject to misinterpretation.

The studies of achievements in Volume II do not justify any belief that education cannot increase achievement nor that there are not possibilities of discovering more effective educative means. When the conditions surrounding the studies in Volume II are carefully appraised they indicate that the influences of differences in the factors studied should be small, as they are. The fact that they are small, or even zero in some cases, does not imply that education is unable to influence the rate and limits of learning. For these statements the following considerations are offered.

First, the various studies in Volume II show the effects upon achievement not of the difference between zero amount and a large amount of some one factor such as time, money, teaching ability or what not but merely the effects of the small differences between two rather large amounts of one of these factors. The reference, quoted above, to the Breed experiment upon pecking which compared much practice but little or no practice is quite misleading.

Second, the difference in the amount of the educative factor under study is probably really very slight when it is realized that this difference appears in comparison with the combined effects of all other influences including a large amount of the factor being studied. The

groups compared in these studies differ in only a small percentage of the total educative influence at work and not at all in the many other factors. Since the groups studied differ only in a small amount in educative influence small differences in achievement should be expected.

Third, it is possible that since courses of study in the three R's are rather rigidly prescribed, extra amounts of time, money, teaching skill, etc. when available, are applied not to increase ability in these subjects but to other purposes. On the other hand, in cases of loss of time and the like it is possible that the first thing made up is instruction in these subjects. It is possible, in other words, that achievement in the three R's is least effected by variations in the educative factors studied.

Fourth, it should be noted that the studies are concerned with small differences in single factors and that were all of these single influences combined, the effects upon achievement might be quite great.

Fifth, it should be noted that the *Yearbook* gives little attention to those factors which laboratory studies show to be most influential, namely, differences in the organization of materials and the technique of learning.

Many laboratory and a few school room investigations show that methods of teaching and learning are potent and that when compared under well controlled conditions they are of very different potency. By one illustration it may perhaps, be possible to show these facts and to indicate a caution needed in interpreting all such studies. To learn a series of symbols frequently used in laboratory studies by the somewhat crude methods we are likely to adopt without tuition requires, let us say, one hundred minutes. If the average learner adopts the single device of working against time, he will realize a saving of twenty per cent of the learning time. The single device of studying the material as a whole, instead of parts brings a saving of fifteen per cent; the single device of recalling instead of rereading yields a saving of thirty per cent; the single device of employing a learning period of optimum length saves fifteen per cent; the single device of optimum interval between repetitions saves twenty per cent; the single effect of certain subtle means of relating the items saves ten per cent of the time. *Add these single savings together and the child learns the lesson not in one hundred minutes but in ten minutes less than no time at all.*

Certainly variable educative factors are not impotent. Of course, the effects of single factors cannot be added in this way. The returns from each are diminished when others are employed simultaneously. Yet, when several crude devices of these types are introduced in organized form into a regular school course, as in studies of reading, arithmetic, typewriting, and some other subjects, the saving over current school room methods has been as great as twenty to forty per cent. The evidence that proficiency may be increased by increasing the rate of learning or by reducing the time required to reach a specified, attainable level of efficiency is appreciable. The evidence is, in other words, that the rate of learning is determined in some measure, and may in the future be determined in greater degree, by the methods of teaching and learning as well as by native capacity.

We must be very clear on this consideration of the rate of learning. The studies in the *Yearbook* clearly show that, taking educational influences as they now are, native endowment results in different rates of learning. The well endowed learn more quickly than the less well endowed. This will be the case, almost certainly, whether all children are taught by, and learn by excellent, mediocre or poor techniques; whether they learn a great or very little. But, the *Yearbooks* give us absolutely no indication whether typical methods of teaching and learning now in use are quite as good, or one-half, or only one-tenth as productive per unit of time, as they might (or will) be after another quarter century of scientific study. A scattering of studies in the psychology of learning make me, at least, quite optimistic concerning the possibilities of increasing the rate of learning and nothing appears in Volume II which disturbs this optimism.

The last question, of paramount importance to all people interested in education and the democratic theory of rule by the majority, is whether by educated influence alone it will be possible appreciably to raise the *limits* of intellectual functioning and achievement. Frankly, we do not know. So far as I have been able to see, only one study in the *Yearbook*, the writer's article in Volume I, deals specifically with this problem. Inasmuch as limitations of facilities forced the investigator to utilize functions of a simpler and narrow type he would scarcely venture to apply the results with confidence to the higher mental operations involved in dealing with abstract and complex ideas. The fact that the *rate* of learning has been shown to be in some measure a function of the technique of learning and teaching, as well as of native endowment, may be taken as at least suggestive of

the possibility that the *limit* of achievement may also be extended by educative factors. The studies in the *Yearbook* show that mere increases of the length of the school day or year, of study periods, of money, of teachers' personality as now appraised and the like are not as hopeful means as those indicated by the discoveries of experimental psychology, namely certain forms of selecting, arranging and organizing materials to be learned and of advancing in the direction of knowledge concerning the more subtle forms of learning and teaching.

In conclusion: It is certain that there are great possibilities of saving time in reaching the levels of efficiency in intellectual operations now attainable by any pupil. It is not impossible that improved methods of teaching and learning will enable any pupil in the time now spent in school to reach a higher level of proficiency. The latter possibility represents, in my belief, the most insistent of current problems in education. Indeed, I hope, that, in the not too distant future, we may have of this topic, *Yearbooks* as rich in materials and as brilliant in execution as the two volumes of the present year for which Professor Terman and his associates deserve unstinted praise. Had I been asked not to criticise but to praise these volumes I should have had much more to say, and I should have spoken with much more conviction, than I have in offering my interpretations of the materials.

EDITORIAL IMPRESSION OF THE CONTRIBUTION TO KNOWLEDGE OF THE TWENTY-SEVENTH YEARBOOK

GUY M. WHIPPLE

Secretary, National Society for the Study of Education

The aim of the Society's Committee on the Possibilities and Limitations of Training has been set forth by the chairman of the Committee, Dr. Terman. The aim has been to use objective and exact methods for securing new facts with respect to an issue which is debatable and controversial. It seems to me desirable to distinguish the issue from the method of treating it. The issue with which the *Yearbook* Committee is dealing is that of heredity and environment, of nature *vs.* nurture, is age-old, and full of controversial possibilities. The method of the Committee, however, has been to avoid mere controversy, to seek for facts, to use the best experimental and statistical methods now available in our profession. This is my first editorial impression of the *Yearbook*—the serious, dispassionate, objective spirit in which the problem is attacked. We have had *Yearbooks* conceived in other veins, collections of opinions, pages of speculations, airings of personal views. I am not saying that these are not desirable and necessary, especially for certain types of problems. I am merely calling your attention to the different aim and spirit in the Twenty-Seventh *Yearbook*. Naturally, those who read the *Yearbook* will continue to differ in their interpretation of the facts that have been marshalled there; that is unavoidable, and not desirable to avoid if it could be. The complexity of the problem is such that the differing lines of attack upon it have of necessity produced data that just now seem somewhat equivocal, and different readers will of necessity differ in their appraisal of the methods that have been followed and particularly, I think, in their appraisal of the presuppositions upon which experimental work must, after all, be based.

My second impression (perhaps it hardly needs the time I feel like giving to its statement) is that we must now see clearly, after reading the *Yearbook*, if we did not see clearly before that, that the problem of heredity and environment, of nature and nurture, is a problem of *degree*. There is no sense in talking about nature *vs.* nurture if we mean to imply that we must choose between them as complete explanations of human life. It is only the beginning student who naively asks his college professor: "Do you believe in heredity or do you believe

in environment?" (I have often been tempted, when this question was asked me, to tell the student about the little boy who was once watching the frying of doughnuts in our house. Seeing his evident interest, the cook said: "Does your mother make doughnuts, Leslie?" to which, after a moment's reflection, the youngster replied, in his boyish lisp: "Thum times the doeth and thum times the doethent." That's about the sort of reply that suits the question: "Do you believe in heredity, professor?")

Thus badly put, the point seems obvious, yet it may bear further scrutiny, after all. The *Yearbook*, despite its voluminous nature—some eight hundred pages of meaty material—is really limited to a consideration of the relative effects of nature and nurture in two chief aspects—their effects upon what we term "general intelligence" and their effects upon achievement in the more readily measurable school subjects. Assume, for the sake of argument that the thirty or more experimental contributions embodied in the two volumes have demonstrated that training has no effect upon intelligence and relatively but a scant influence (let us say, ten per cent) upon achievement in the school subjects. Does it then follow that training is substantially a negligible factor in the production of adult mentality? Of course, it does not, and for several reasons.

For one thing, the hereditary influence, even it were overwhelmingly preponderant, is exerted and displayed only when the individual is living in an environment, when this environment is constantly stimulating the individual's innate, constitutional capacities. Even a Boston baby, transported to Zululand and reared with the Zulus, would doubtless fail, as an adult, to become an ardent worshipper of the music of Brahms (though he might enjoy a good tom-tom), and he would presumably not be heard discoursing effectively upon the merits of Freudian psychoanalysis.

But for another thing, leaving this matter of the necessity for interaction between heredity and environment quite aside, the contributions to the *Yearbook* have been limited to the study of certain phases only of mentality. Thus, there has been left wholly out of account (save for one chapter by May and Hartshorne) the issue as to the relative influence of heredity and environment upon those non-intellectual aspects of mentality that we roughly designate by the terms "personality" or "character"—an issue of the highest practical social importance—the "possibilities and limitations of moral training," if you prefer that formulation of it. Likewise, the Committee

has made no attempt to discuss the relative influence of heredity and environment upon the entire physical make-up of the individual. Similarly, with respect to numerous special abilities or talents, though the Yearbook offers some evidence concerning the factors lying behind one such ability—mechanical ability; that is the only contribution in this field with definite results; the summary of the issue so far as musical ability is concerned only serves to indicate the complexity and uncertainty of our knowledge.

If, then, we propound such questions as: Can training develop resistance to tuberculosis? Can training thwart impending psychoses? Can training alter temperamental tendencies? Can training eradicate racial prejudice? Can training make it possible for every intelligent child to sing on pitch? Can training imbue a nation with lofty ideals? If we propound these and dozens of other similar questions, all nature-nurture problems, we shall have to say that the *Yearbook* we are considering this evening has had, perforce, to neglect them entirely or to touch upon them only inferentially.

The point I am striving to make—perhaps not very effectively—is that the various human traits do not behave alike, that it is wise to think of them as open to training in different degrees, so that the determination of the relative influence of heredity and environment is not something that can be made once and for all, but something that must be made independently for each one of the numerous traits that we strive to influence in the home and in the school. Moreover, in practically every case, we shall not expect to say training is not effective or training is effective, but rather, training conducted at such and such an age, by such and such methods, and continued for so long a time, may be expected to produce an alteration of approximately so much in thus and such percentage of children. You may smile at this statement and say: "How like a college professor; you can never pin him down." It isn't the college professor who can't be pinned down; it is life itself that resists sweeping conclusions and pat generalizations. Human engineering has to deal with qualifications, with contingencies, with probabilities, however annoying these may be to the seeker after precision and finality.

I have found it instructive in this connection to speculate in this wise. Suppose a line ten inches long labelled zero per cent at the left end and one hundred per cent at the right end—meaning zero per cent and one hundred per cent, respectively, susceptible to training. Next draw up a long list of human traits, physical, mental, moral. Then

try to place these traits where you think they belong on the line. Get some equally sagacious friend to do the same thing with his ten-inch line, and compare your results. Here and there you will find yourself in agreement, but only here and there, and your divergencies will furnish matter for argument till the cows come home—that's why we have a time-limit on these addresses to-night! Just two or three examples: Where do you put eye-color? Undoubtedly, at the left end of the line. No one expects to alter eye-color by environmental agencies—no one, anyway, but the more rhapsodical novelists! Where do you put the ability to spell cat? At the right end of the line—or perhaps nearly there, for you make a mental reservation that idiots can't be taught to spell. Where do you put ability to see fallacies in the arguments of demagogues? Somewhere below the middle, perhaps. Where do you put general intelligence? Now we come, you see, to a question that the *Yearbook* does deal with and one that leads me back to my theme "The Contributions to Knowledge of the Twenty-Seventh *Yearbook*." It is well to return to our topic because I have mentioned so many aspects of the nature-nurture problem *not* treated in the *Yearbook* that I may have seemed to speak derogatorily or apologetically of it. That is not my intention, not in the least. I have been trying to make clear merely that the Society's Committee has been compelled to restrict its first report to the possibilities and limitations of training within certain circumscribed portions of human activity.

Yet these portions are of exceeding importance to educational theory and practice. The first issue—that dealt with in Part I—is, in substance: Does training affect general intelligence? Or, more accurately: What are the possibilities and limitations of training upon general intelligence?

"More accurately" because we should perceive clearly that once more the issue is not: "Do you believe in heredity—with respect to general intelligence?" Here, once more, the question is one of *degree.* No one who reads the *Yearbook*—this is my impression at least—can put it away with the conviction that general intelligence is an absolutely fixed, immutable, innate capacity, but neither can one put it away with the conviction that general intelligence is readily susceptible to environmental influence. The truth lies between these two extremes.

Moreover, I think it is worth noting that the *Yearbook* contains almost no discussion of what general intelligence is. That controversy

appears to have been thought of as a somewhat academic debate—not necessarily a closed debate, but a debate not particularly affecting the argument before us. Rather, the contributors have practically all said or implied: I mean by general intelligence the mental capacities that are measured by the best intelligence tests, especially by the Binet Scale. I believe that the contributors would be willing to say: "Yes, I'll call this 'native intellectual ability' or 'capacity to learn' or 'scholastic aptitude' if you prefer those terms to 'general intelligence.'"

It is my impression, therefore, that the contributions to Part I of the *Yearbook* may be said to have provided new facts, extremely important facts, bearing upon the issue: "To what extent, and under what conditions, do environmental factors modify the child's natural scholastic aptitude?" Naturally, the contributions are not all in exact accord—no one would have expected them to be. Naturally, they are not of equal importance or of equal merit as conclusive scientific material; no one would have expected that, either. Two of them—the foster-children studies of Burks and of Freeman and his associates—must surely be given the right of way as the contributions that have driven most directly and with the most adroit use of modern statistical methods toward the heart of this main issue.

Opinions will differ as to the interpretations to be given to these two contributions; they coincide in some respects, but they differ to some extent in their conclusions as to the degree of the environmental influence which they measured. Since Dr. Freeman has already considered this problem of evaluation and since he knows far more about it than I do, I shall not enter any discussion of these two chapters at this time. It has occurred to me, however, that I might attempt to put before you, quite sketchily but as fairly as possible, the general trend of the remaining contributions of Part I. The *Yearbook* is so technical—almost "high-brow," one is tempted to say—that an attempt to condense its conclusions will perhaps be welcomed by the members of the Society. This, then, is the way the contributions impress me.

Thorndike begins by finding a correlation of .60 between the intelligence of unselected siblings. Karl Pearson had previously discovered a correlation of about .52 between the physical traits of siblings. Assuming that intelligence and physical traits exhibit about identical hereditary tendencies (for which there is some evidence), it would follow that the similarity of the environment that exists between siblings two to four years apart in age in an American family today is

sufficient to raise the natural correlation of their intelligence scores from .52 to .60—not very much, I think you will agree.

The assumption just mentioned concerning the close correspondence of hereditary influences for intelligence and for physical traits is supported in the next chapter by data contributed by Jones.

A clever use is made by Miss Koch (Chap. VI) of the existence of Siamese twins. One outcome of her measurements of such a pair of twins is that the amount of divergence they show on standard intelligence tests is as large as the amount usually found between ordinary identical twins, despite the fact that it would be hard to think of any arrangement that would come nearer guaranteeing identity of environment between two children than hooking them together as Siamese twins are hooked.

Continuing this line of argument, Miss Tallman shows (Chap. VII) that in terms of likeness of scores in intelligence tests, the four possible kinds of siblings stand in the order: (1) Identical twins, (2) like-sexed twins (which of course, include all the identical ones,) (3) non-identical twins, and (4) ordinary siblings not twins. It is surely difficult to explain this order by any theory that discards heredity as a factor conditioning intelligence scores.

Burlingame and Stone next show (Chap. VIII) that, even in the case of white mice, a correlation of .31 exists in the abilities of pairs of siblings to run through mazes.

Miss Rogers and her associates at Bryn Mawr find (Chap. XII) that when children who have been living in a distinctly unfavorable environment are transferred to an exceedingly good and well-managed institution, their school achievement improves after a year or more of residence, whereas their intelligence, as indicated by Binet IQ's, shows no evidence of improvement.

Peterson (Chap. XIII) finds that the differences repeatedly discovered between whites and negroes when given verbal intelligence tests likewise appear when they are given his Rational Learning Test, although that depends very little upon facts and skills accumulated through schooling or from the social *milieu*.

Similarly, the Misses Murdoch, Maddow, and Berg find (Chap. XIV) that the amount of English spoken in the homes of seventh-grade Jewish girls bears no relation to their scores on verbal tests.

A study by Miss Hildreth (Chap. XV), limited to forty-eight pupils, indicates that the six points of IQ advantage exhibited by pupils entering the first grade from the nursery school or the kindergarten is

partly cancelled later by three points of gain in the other pupils which is not paralleled by further gains in the nursery school and kindergarten pupils.

Miss Goodenough (Chap. XVI), attacking a similar problem, finds a zero correlation between gain in IQ and length of nursery-school attendance.

The Misses Hoefer and Hardy (Chap. XVII) find very little tendency toward improvement in either IQ or EQ in three hundred forty-three elementary-school children of Joliet, Illinois, as a consequence of specific measures taken to improve their physical condition (tonsils, coffee-drinking, etc.).

Stone, at Stanford (Chap. XVIII) and Gesell, at Yale (Chap. XIX) both report that in such a pathological condition as puberty præcox there may occur extraordinary acceleration of sex development without any corresponding acceleration of mental development.

Miss Matthew and Miss Luckey (Chap. XX) find that, of thirty-eight cases in which the IQ changed as much as five points on retesting, thirty-four were readily explicable as caused by medical complications, psychopathic tendencies, behavior difficulties, or linguistic difficulties.

Mrs. Greene (Chap. XXI) finds that while, as one might expect, direct coaching on the Binet tests produces a decided rise in the IQ gained, coaching on related material produces only four or five points gain (about the amount of the probable error of the IQ), and the effects of either kind of coaching are not permanent.

In the final chapter (Chap. XXIII) Gates declares that "the effect of education is not to change the growth of capacity, but only to give the recipient useful information, techniques, methods of work, and the like."

Here, then, are sixteen chapters, undertaken independently by more than sixteen different investigators. It is my impression, and I believe it will be yours, that the trends of their data lie very clearly in the same general direction.

Time forbids my attempting to summarize the evidence presented in Part II of the *Yearbook*. It is my impression that the general trend of the data assembled in that part is to emphasize the preponderant rôle of intelligence in conditioning school achievement. Compared with intelligence, such factors as length of school attendance, length of the school year, per capita cost of instruction, time devoted to study, ability of the teacher (within the limits of variation usually found for those factors) compared with the intelligence of the pupil, these factors

singly and even all of them collectively play a minor rôle in determining measurable achievement in the classroom. Such is the general outcome of Part II.

All in all, therefore it is *my* impression, that, within the limits of human mentality with which it deals, the Twenty-Seventh *Yearbook* strengthens the contentions of those who, speaking in hyperbole, are termed "hereditarians." The *Yearbook*, so far as I can perceive, affords not even a single straw that can be clutched at by those extreme environmentalists who deny, *in toto*, the inheritance of intellectual disposition, and who would have us educational workers accept the staggering doctrine that, if we only knew our business, we could make of any child whatever we wished—even to the fashioning of the proverbial silk purse from the sow's ear.

In closing, let me emphasize one editorial impression in especial. This one is nothing novel. Dr. Terman made the point in his introductory chapter, and in fact I have touched upon it already myself. I refer once more to the dispassionate, serious effort at objectivity that has animated the Committee and the contributors. My plea is that we, in reading, in appraising, in discussing this *Yearbook*, should likewise strive to maintain the attitude of dispassionate objectivity. There are, in the Twenty-Seventh *Yearbook* interpretations of data, formulations of conclusions, that will run counter to the assumptions, and even counter to the convictions, of many in this audience. So much the greater, then, is our obligation to adopt a neutral attitude. More than that, it will be better for us to make believe for a few minutes that we have been wrong if we are to get from this *Yearbook* a just appreciation of its contents. Let us, then, in our consideration of these investigations, be open-minded. Let us not fear to face the consequences that they may have for our educational thinking and practice. I am told that, not so many years ago, Dr. Rice was openly jeered at when he told an audience of the National Education Association of the results of his pioneer investigations of spelling. Whether that is true or not, the time for that sort of reception of educational investigation has passed. We cannot adopt the attitude of that German philosopher who, when told that the facts did not accord with his theory, retorted: "*Um so schlimmer mit den Tatsachen*"—"So much the worse for the facts." We members of the National Society are dedicated to the scientific approach to the Study of Education.

INTELLIGENCE AS METHOD OF ADAPTATION

CHARLES H. JUDD

University of Chicago

The most impressive fact with regard to this *Yearbook* seems to me to be that the two major investigations, one conducted at Stanford and the other at Chicago, resulted in an agreement. To quote Miss Burks' statement made on page 318, "The investigations agree in attributing small, but significant increments of IQ to superior environment." Such a pronouncement made by a representative of Stanford cannot fail to effect profoundly the thinking of the educational world. It has been commonly believed that the Stanford group regards education as the exclusive right of those whom heredity has made competent and that intelligence is a product of heredity. If it is now agreed by Stanford that a superior environment can produce a positive change in the IQ, the educational program of the country can, with full assurance, continue its effort to provide a superior environment for everybody.

There are many of us who have felt clear that there is a demand in America for the best possible environment for every child and we have watched with some anxiety the spread of what appeared to be a fatalistic theory among those who have adopted without reservations the view that the IQ never changes. Now that the leading investigators are all agreed that a superior environment improves the IQ, we can organize the schools without any of the inhibitions which arise from a false view regarding the absolute fixity of the IQ.

There remain, of course, a number of problems on which there is still lack of complete harmony. For example, it is difficult to find any real justification for the effort of the Stanford investigation to put into numerical terms the relative importance of heredity and environment. In her summary on page 308, Miss Burks ventures the statement that, "on an average, less than ten points of IQ must be due to environment." The figure "ten points" is derived from a series of calculations which throw together data that are so heterogeneous in origin that they might better have been, in my judgment, thought of as relatively loosely related until the discussion can be conducted on a much more mature basis. Furthermore, I find this kind of effort at quantitative determination likely to be altogether misleading. Sup-

pose that it ultimately can be shown with infallible certainty that a superior environment is capable of increasing the IQ by ten points, what does this mean? I think of a mechanical analogy. An engine is drawing a train at the rate of forty miles an hour and by the use of more fuel and better methods of operation the speed is increased by ten points. Are these last ten points like the first or second units of ten? Or consider, if you prefer, the engine's start. When it pulls the train from zero speed to ten miles an hour, is the achievement the same as when this same engine adds ten miles speed to the first ten and so reaches twenty miles an hour? The mechanical analogy may not be perfect but it expresses in a concrete way the objection which I find to the statement that heredity supplies seventy per cent or eighty per cent of intelligence and environment supplies ten to twenty per cent. If the educational environment, when properly organized, can in a single lifetime speed up intellectual adaptation to a degree equal to one-seventh or one-eighth of that which is supplied by the long processes of human and animal evolution, we may well think of environment as of stupendous importance.

A second point at which disagreement appears is as follows. Discussion in the past, and to some extent in the present, seems to me to be obscured by the overemphasis which the hereditarians have given to the physical transmission of traits through the bodily organism. It is certainly a matter of major importance to human life that the normal individual comes into the world with eyes, ears, and vocal cords and of finger muscles which are capable, after a time, of wielding tools and of forming the letters of the alphabet. No one will deny the importance of these physical traits. There is in the world, however, a freightage of the first importance to human life which the bodily organism cannot carry. Physical heredity is impotent to transmit language or science or art. Evolution of the type which is essential to the perpetuation of civilization requires the handing down from generation to generation of literature and number and governmental policies. The germ plasm cannot carry these and, therefore, it has been necessary in the course of the world's progress for channels of transmission other than those of physical heredity to be devised and conserved. You and I did not inherit the English language through physical transmission of traits. We have, however, since the date of birth acquired, in addition to the powers which nature gave us through physical heredity, certain modes of behavior which have made us what we are as civilized human beings. We have passed, since we were

born, through a series of experiences which are quite as important in determining our thinking of tomorrow and the next day as are any of the experiences through which the germ plasm passed before birth. We have inherited much from the efforts of those who through long decades have perfected and recorded the English language and have thus given to each of us an instrument for the acquisition and expression of ideas which is superior to any device of communication that has ever appeared in the history of the animal world. Language is transmitted, not through germ plasm but by a method of evolution which produces adaptations utterly beyond the reach of germ-plasm transmission.

The effort to lump together physical heredity and such devices of adaptation as language, and number, and science leads to endless confusion. Language is an acquired form of skill. Why should any one overlook language in cataloging the inheritances which come to us from our ancestors? To be sure, language comes to each one of us by a process other than that by which we inherit eyes, and ears, and vocal cords, and hand muscles. To be sure, languages in different parts of the world differ more than do the physical traits of human beings, but that only goes to show that language is a highly complicated function which physical heredity cannot carry. What physical heredity cannot carry must be transmitted by some other channel. Handwriting, like spoken language, was never inherited by any human being and yet language and writing have transformed human existence. They can properly be described as fundamental methods of civilization. In like fashion, we may catalog a long list of methods of thought and investigation and we may properly say of them that they are products of social inheritance.

Let us put the matter in terms of a concrete comparison. Suppose that a member of modern society had to choose between, on the one hand, an IQ of ninety plus such knowledge as he can absorb of all that modern science can teach and, on the other hand, an IQ of one hundred and such limited knowledge as Robinson Crusoe and his man Friday could in their isolation accumulate without the help of the teachings of science, which would he choose as more likely to contribute to a successful career?

I did not institute the comparison which I have just suggested without full realization that the extreme advocates of the absolute fixity of the IQ provide themselves with a way of escape by saying that intelligence is the capacity for taking from the environment all

that is desirable. Their very definition of intelligence, therefore, absorbs the environment into what they call intelligence. They then turn on environment and call it a matter of very minor importance. They admit that it requires intelligence to absorb science but they seem to overlook the fact that the processes by which science has been produced are manifestations of intelligence and that science is the carrier of devices of adaptation which have completely transformed the natural environment. Science is certainly something different from the natural environment. Why should we not recognize that it is a body of methods of adaptation which, when transmitted, constitute a part of the individual's equipment no less than do his physical traits? The extreme herediterians commit the fallacy of thinking so intently about one form of heredity that they overlook social heredity. They fall into the grievous error of saying that social intelligence is not worth much when measured by the yardstick which they have devised.

For my own part, I find it to be much more rational to think of heredity as a compound concept and to think of intelligence as a name for a group of human traits. I find my educational creed made clearer when I think of language, and number, and science as a part of the world's intelligence and as forms of personal experience which are acquired after birth, but are no less inherited from earlier generations than are physical traits.

I can make clear what I have been saying by the use of certain illustrations. Foreign language teachers who know the laws of phonetics are able to teach their pupils more exact forms of pronunciation than they themselves possess. In other words, the competent teacher knows something more than pronunciation. He knows the methods of pronunciation. When he teaches his pupils, he directs their development. He produces in them modes of behavior which are more highly perfected than are any instincts which they inherited through the germ plasm. If the hereditarians have a place in their definition of intelligence for those forms of behavior which come through the germ plasm, why do they exclude from their definition of intelligence forms of perfected pronunciation imposed on the organism through the training given by a skilful teacher?

As with the teacher of pronunciation, so is it with every other teacher who is striving to direct the new generation. It is the hope of every thoughtful teacher that he may train his pupils in methods of intellectual adjustment which will carry these pupils further than the

older generation has been able to go. The chemist in his research laboratory teaches methods of analysis and synthesis and leaves it for his intellectual descendants to increase the world's intelligence and to add to the world's knowledge with regard to chemical processes through the use of these methods.

There is likewise an endless chain of examples of great historical acquisitions which have transformed the world's methods of intellectual treatment of the environment. The Chinese have a clumsy method of writing. On the other hand, the Occidental world borrowed from some Semitic tribe an alphabet which is infinitely flexible. Was the invention of the alphabet a contribution to the world's intelligence or not? I think the answer is emphatically in the affirmative. The alphabet has done as much to transform the world as has any change known to science as having occurred in the germ plasm of any individual or group of individuals.

As I see the matter, those who think of the IQ as an index of that which comes to the individual through physical inheritance are very short sighted; they overlook entirely the broad historical movements which go to explain modern advances in intelligence. Of course, the band of out and out believers in the IQ as a fetish to be worshipped and thought of as the sole guide of educational policy must be getting uncomfortably small now that the Stanford group recognizes the fact that significant changes take place in the IQ as a result of the action of a superior environment. It is my hope that the Stanford group may be persuaded to continue their researches and I have a strong impression that if they continue, they will find grounds for extending the definition of intelligence to include some of the higher powers of thought as conditioned by language, and number, and all that comes after one begins to use his eyes and ears.

There are a number of other matters which attract the attention of the careful reader of the *Yearbook*. I shall call attention to two such because they seem to reinforce very powerfully the position which I have been defending. Chapter XVII by Misses Hoefer and Hardy seems to me to be a very impressive chapter. Briefly put, we may summarize that chapter by saying that pupils who are in good physical condition are likely, on the whole, to show IQ's superior to those shown by pupils who are less favored physically. The advantages of good health do not seem to appear suddenly. Good or bad tonsils are not closely paralleled by high and low IQ's and broad shoulders and satisfactory physical weight are not necessarily connected, so far as the measurements show, with high levels of intelligence.

The meaning of these facts seems to me clear. Good health is favorable to high IQ's because in general if the physical mechanism is impaired, the nervous system functions deficiently and if the physical mechanism is working normally, all the individuals' functions will show favorable effects. On the other hand one unfavorable physical symptom is so insignificant as contrasted with the immense importance of the school arts that there appears to be no relation between intelligence and any particular physical trait. Tonsils, to be quite explicit, are important but they are totally eclipsed by lessons in arithmetic. Broad shoulders are desirable but if one can read, he will be able to get on very well with shoulders that are not up to par. Even people who are underweight may take courage from the measurements of Miss Hoefer and Miss Hardy. They may some day be known as competent citizens if they will only put their faith in the English language and the Arabic numerals.

I would not have any one misunderstand what I am saying. I believe in tonsils and in proper adjustments of weight. I believe in shoulders and lungs. I believe equally in literature and science. My definition of intelligence is based on the observation that the world has made progress in accelerated degree since the time when literature and science came to be recognized as social inheritances of prime importance to the world. I would not commit the fallacy of putting into figures values for which I have no exact measures. If any one were to force me, however, to decide how many points in my IQ are directly traceable to the Arabs who, so far as I know, are not in the direct line of my physical ancestry but who gave to my European ancestors a number system, I should be compelled to be liberal with these same Arabs, far more liberal than ten points.

A final contribution to my social inheritance drawn from the *Yearbook* was derived from a study of the chapter by Professor Gates.

This chapter seemed to me to be impressive first of all because it is a report of an experimental study. When science encounters a highly complicated situation, it always begins its attack on this situation by making an analysis and by isolating in this way the factors with which it has to deal. Professor Gates has made an elaborate analysis of his problem. In so doing, he has set a helpful example for the science of education. Careful laboratory experiments are much more arduous than is the wholesale giving of tests. Because tests are relatively easy to administer, we have had a flood of superficial studies which have employed tests without analysis and have reached conclusions

which are painfully trivial. Professor Gates has used tests but his work is that of a laboratory scientist.

He experimented with two simple functions, namely, tapping and immediate memory. He found that the effects of training were in the first case, that is in improving the rate of tapping, very slight. He found in the second case, that is in immediate memory for numerals, that training produces no permanent effects. He concludes from these experiments that the effects of training are in general due to the cultivation of certain techniques or certain tricks of efficiency rather than to changes in fundamental capacities.

I do not know how far Professor Gates will be willing to accept what I have called "social intelligence" among his tricks or techniques of adaptation, but to my mind the implications of his experiments are clear. Wherever a pupil finds it possible to adopt a new method of behavior, he has introduced into his life a real factor. If the method of behavior is suited to the situation, the IQ goes up. If the method is unsuited to the situation, the IQ goes down. Method of response or method adaptation is a highly important matter and it is this method of adaptation which education aims to guide.

I could wish that Professor Gates had given more attention to some of the higher forms of learning where method of reaction is more significant. There are in the literature some very good examples of investigation which he has overlooked. Sooner or later the list of studies of higher forms of learning will, I dare say, be supplemented by his own investigations. The higher forms of learning are impressively dependent on the acquisition of techniques of reaction. A study of the effects of training in the methods of science, for example, will prove very revealing to Professor Gates when he reaches the point where he is interested in the relative value of theory and practice and he will find ample verification of the view that it is the techniques of procedure which are the real ends of education. I think he will discover also, as has been pointed out in some of the reports of earlier experiments which he overlooked, that these methods of intellectual procedure can be generalized and can be formulated into what we call laws in science.

What I have been saying may be summarized as follows. Throughout this *Yearbook* there is large encouragement for those of us who believe that education is capable of improving individuals. A favorable intellectual environment does something to pupils. We must find by careful experimentation how to produce and apply favorable

environments. We must find what methods of adaptation the race has discovered and we must add these to a pupil's physical equipment. This *Yearbook* ought to put an end to the superficial fatalism that has been rampant in certain quarters and ought to turn educational science into an experimental endeavor to discover how to transmit social inheritances as readily and as completely as possible to all pupils. When we thus understand the function of education, we may confidently define the ends of education by saying that individuals are what they are because of what they inherit from their germ plasms and from their social surroundings. We may think of their function in the chain of generations to be first the acquisition of the best known methods of reaction and second the initiation of new and better devices of adaptation to be transmitted through education to the next generation. The word "intelligence" will then include many different kinds of acquisitions and many different forms of initiative and the IQ will be a tool of education rather than a deadening and inhibiting influence on school organization.

HEREDITY AND ENVIRONMENT—OR CAPACITY AND TRAINING?

BIRD T. BALDWIN

Iowa Child Welfare Research Station, State University of Iowa, Iowa City, Iowa

Dr. Terman, Dr. Whipple, and the other members of this year's Committee for the Twenty-seventh *Yearbook* are to be congratulated on discovering and assembling so many significant studies in the field of experimental education. This extensive series has sunk a shaft far below the surface into some of the basic principles of mental life and child training; many new facts, and particularly many new implications and applications, have been unearthed. These two volumes are going to evoke a great many vital questions; good experimental studies usually do. Personally I have never felt cause for alarm from good scientific experimental procedure, so long as the methods, technique, and data are open for further verification or disproval. I believe that these volumes, like those on reading and the curriculum, mark a very signal step in advance in the field of education. I also expect our next *Yearbook* on preschool education to continue in this problem of the nature and nurture of young children as a further experimental contribution to the general field of educational procedure.

Since at our meetings the other members of our committee have criticized the specific contributions in the 1928 *Yearbook* and since we read and criticized only the original manuscripts of the experimental studies, I am going to take advantage of this opportunity not only to give my impressions but also to take a forward look on the nature-nurture problem and, particularly, on the future of education.

Of the fact that this year's volumes have their limitations we are fully conscious, and of the fact that we have made a certain amount of selection due to availability of manuscripts, we are also conscious. But the limitations of this series of studies are mainly those of the science of education today.

Education has not grasped some of the fundamental concepts of scientific research. It is overlooking the inter-relation, the coördination, and the integration of the natural and social sciences, although it is making good use of educational psychology and statistics.

This book has made a constructive start in one direction, that of assembling experimental studies in education. The investigations, particularly those of nature, are restricted in certain particulars to an

assumed infallibility of the intelligence quotient as a measure of nature. Several of the writers seem to assume that the ratings of a few imperfect intelligence test scores stand for a very intricate, complex, and unchangeable phenomenon back of the scores—intelligence—with which individuals are endowed at birth. The IQ is posited as the nature factor, pure and simple. The IQ is a formula that represents something that is measured by a particular series of empirical tests and chronological age. But chronological age is rapidly becoming discarded by biologists and physiologists as an accurate index of maturity or development, and the intelligence tests are not claimed by psychologists to be accurate measures; they are not very good at the earlier or later ages; they are not designed for repetition; their application may be easily blocked by emotional disturbances; and they are apparently much influenced by educational experiences. In fact, one of the best authorities in the field, Kelley, believes that for general grouping purposes in a school the Stanford educational achievement scale measures practically the same abilities, and may profitably be substituted for the intelligence tests.

My first criticism of our book is that we have given a very restricted interpretation of nature. The general title of the book, Nature and Nurture, is a misnomer. These studies lack appreciation of the rich field of research in heredity, in the fields of genetics, biology, physiology, embryology, and anthropology. There are no references in the context and the bibliography to the experimental investigations of Boas, Hrdlicka, Wissler, Benedict, Castle, Jennings, Kellogg, Parker, Pearson, Pearl, Evans, McClung, Swingle, Scammon, Stoddard and McCullom, and but one writer refers to Mendel and then only in a casual way. Two of Davenport's minor studies are cited among the references.

Such biologists as Conklin, Morgan, and Mueller show that even the germ plasm does change under certain environmental conditions, and that individuals can be changed for at least one or two generations. It is even probable that the immediate environment of the germ plasm may determine the sex of the individual. Mueller, through the use of the X-ray has been able to speed up heredity one thousand per cent, he claims.

In biological and physiological laboratories much interesting work is being carried out with animals that no doubt has its human applications. Our biological laboratories at Iowa, like those in other institutions, are demonstrating that rats can be so modified by lack of

vitamins that they are unable to reproduce in three generations; tadpoles in whom the thyroid has been removed remain tadpoles instead of becoming bullfrogs. If thyroid extract is injected, the growth process is accelerated by three or four months. If the degenerating gill slits of the tadpole are transferred with the skin area to the middle of the back, the tympanic membrane appears in the middle of the back with evidences of nerve connections instead of in the side of the head.

In a recent conversation in the laboratory of a brain surgeon, we discussed the fact that a few drops of blood injected into the cerebrospinal fluid may throw a guinea pig into convulsions, with accompanying paralysis, while the examination of the spinal fluid of mental defectives and children in after-stages of meningitis shows the presence of a few drops of blood. Where do nature and nurture enter in here?

No one seems to be able to determine what is original or what proportion of maturation is original and what proportion is learning. Only recently Henry Fairfield Osborne wrote: "Constitutional predispositions in the germ-plasm are not released except through adaptive reaction to new conditions of life; they are not therefore, of the nature of inherent perfecting tendency but, rather, of the nature of a potentiality to appear when the need of them arises." No one seems to have proved that the growth of intelligence is independent of maturation of the individual from birth through childhood. Stone and Gesell have shown that accelerated or retarded maturation of a few physiological processes does not necessarily affect certain phases of mental development. Stone does not tell how he measured mental development; Gesell gives as his means of measuring: Drawing, drawing a man, language and vocabulary, digit recall and Knox cube test, number sense, play interests, school report, and reading. Stone does not claim that nature is not modified by maturation and maturation by environment or nurture. Many psychologists have eliminated all instincts from the nature side of the picture. Other psychologists are finding that Watson's four original emotions can now be reduced to at least two. Heredity and environment are integral parts of the developing organism so intimately bound up with each other that a separation, except for mere scientific classification or description, can not be made. It seems that learning and maturation are dependent on each other. That maturation is influenced, accelerated, retarded, and modified by environmental factors is accepted by most of the leading biologists of today.

Since in our intelligence scales and educational achievement scales we are dealing with an individual's capacity at any particular stage of development, would it not be better for us frankly to substitute the terms capacity and training for heredity and environment? By capacity, I mean the measure of the individual's responses regardless of whether they are inherited or acquired. We have standardized scales of many descriptions for measuring physical, emotional, intellectual, educational and, to some extent, social capacities, and by measuring these at stated intervals we can get a real insight into the extent of training and maturation.

Some of us believe that we are on the verge of establishing a new science of child development. We find that a child is something more than an intelligence quotient or eight carpal bones. The personality of a child is a changing complex integrated unity. We are beginning to realize that for many purposes maturation is more significant than chronological age; that intensive studies from different aspects of development indicate complexity rather than simplicity; that different phases of growth are integrated with each other, and the present and future of a child with his past; that unity is apparent as a fundamental characteristic of the organism in the physical life and in the mental life. The general and specific elements of intelligence are integrated phases of the total mental life of the child.

In our efforts to discover the native endowments, or capacity, of the children from two to five years in our preschool laboratories from the standpoint of interests, emotional trends, social attitudes, language, and motor control, we have found that little children's personalities can be modified and improved through modern methods of observation and training if the task is undertaken sufficiently early in their development. New interests can be awakened, undesirable temperaments modified, fear complexes eliminated, social aversion and extreme shyness overcome, language habits completely changed, motor control improved, and vocabulary greatly increased. The IQ's fluctuate and usually show improvement. Nature furnishes some of the elements for education, but environment, nurture, and training are the determining factors in the results.

In a study that we have been carrying on for almost five years of a number of rural communities in Iowa, we have found that the children of the various communities differ from each other and city children in intelligence quotients, and their differences increase with age. There is no statistical difference between the intelligence scores

of rural and city infants; at five and six years of age the rural preschool children are inferior in the kindergarten tests and the form board type of performances. When measured by the Stanford-Binet scale the rural one room school children are inferior to those in consolidated schools and those in consolidated schools to city children. These differences increase with age with marked inferiority in language ability and vocabulary and with superiority in acquisitions of a practical everyday nature. That performance tests measure something quite different from the Binet tests is evident; in certain kinds of performances, the rural children are inferior and in others superior. Rural children differ in intelligence in different communities regardless of economic status. That the background and local traditions, mores, ideals, interest in social and civic movements exert a moulding influence on the minds of children is evident.

The Twenty-seventh *Yearbook*, I am sure we all agree, has not only made a distinct contribution, but is a step in the right direction from the experimental point of view. Our future program, to me, seems clear. We need the assistance of a large number of scientific investigations that have been made in the field of child development.

TWINS AND ORPHANS[1]

A. H. WINGFIELD AND PETER SANDIFORD

University of Toronto, Toronto Canada

Although a large number of studies of twins have been made, the majority of them deal with physiological and surgical aspects of the subject. Only four studies of importance have dealt with the educational aspects of the inheritances of human twins. They are those of Galton, Thorndike, Merriman and Lauterbach.[2]

Galton drew attention to two types of twins, identical and fraternal. He compared a group of thirty-five, reported as being very similar, with another group of twenty distinctly dissimilar pairs. He sent circulars of inquiry to persons who were either twins themselves or near relatives of twins. His data, consequently, are lacking in scientific objectivity, nevertheless his main conclusions have stood the test of time.

Thorndike used objective measurements but his mental tests were the simple ones of the pre-intelligence test era and were not as valid and reliable as those now in vogue. His subjects were fifty pairs of twins chosen at random from the schools of New York City. He

TABLE I.—RESULTS OF MENTAL TESTS FROM THORNDIKE'S STUDY

Test	Twins	Siblings	Younger twins	Older twins
Cancellation of A's	.69	.32	.66	.73
Cancellation of a-t and r-e	.71	.29	.81	.62
Misspelled words	.80	...	.76	.74
Addition	.75	...	.90	.54
Multiplication	.84	...	.91	.69
Word opposites	.90	.30	.96	.88
Average	.78	...	.83	.70

[1] In this paper Wingfield's investigation has been summarized by Sandiford, under whose direction the study was made. A detailed account will be published by J. M. Dent and Sons.

[2] Galton, F.: "History of Twins" in "Inquiries into Human Faculty and its Development," 1883. Thorndike, E. L.: "Measurements of Twins," 1905. Merriman, C.: "The Intellectual Resemblance of Twins," 1924. Lauterbach, C. E.: "Studies in Twin Resemblance," 1925.

employed fourteen tests—eight physical and six mental. Using correlations as measures of resemblance, he obtained for mental traits the results given in Table I.

He argued thus:

> If these resemblances are due to the fact that the two members of a twin pair are treated alike at home, have the same parental models, attend the same school and are subject in general to closely similar environmental conditions, then, (1) twins should, up to the age of leaving home, grow more and more alike; (2) if similarity in training is the cause of similarity in mental traits, ordinary fraternal pairs not over four or five years apart in age should show a resemblance somewhat nearly as great as twin pairs, for the home and school conditions of a pair of the former will not be much less similar than those of the latter; (3) if training is the cause, twins should show greater resemblance in the case of traits much subject to training than in traits less subject to training. On the other hand, (1) the nearer the resemblance of young twins comes to equalling that of old, (2) the greater the superiority of twin resemblance is, and (3) the nearer twin resemblances in relatively untrained capacities comes to equalling that in capacities at which the home and school direct their attention, the more must the resemblances found be attributed to inborn traits.

His interpretation of his results is that "the mental likeness found in the case of twins and the differences found in the case of non-fraternal pairs, when the individuals compared belonged to the same age, locality and educational system, are due, to at least nine-tenths of their amount to original nature." He thus confirmed a conclusion of Galton's from less objective data.

Merriman measured only the general intelligence of twins, using the Stanford-Binet, Army Beta, National Intelligence, and teacher estimates. His correlations are given in Table II.

Merriman concluded that "environment appears to make no significant difference in the amount of twin resemblance. Older twin pairs are no more alike than younger twin pairs."

Merriman, however, except in the case of the Binet results, where IQ's were used, did not eliminate the effect of age on the amount of resemblance found. If unrelated children are paired for age, a significant amount of correlation is found. As a matter of fact Merriman's own data show a correlation of $-.29 \pm .043$ between the chronological ages and the IQ's of two hundred four twins, whose ages ranged from six to fifteen years. Merriman also failed to take into account the variability of his measurements. If this is done, as Shen (1925) has shown, then some of his conclusions are not justified.

TABLE II.—RESEMBLANCE OF TWINS IN INTELLIGENCE AS SHOWN BY MERRIMAN'S STUDY

Group	Binet IQ	Beta scores	N. I. T. scores	Teacher estimates
All pairs, 5–9 years	.809 ± .032	.784 ± .049	.797 ± .034	.686 ± .057
All pairs, 10–16 years	.757 ± .037	.664 ± .054	.875 ± .017	.373 ± .081
All twin pairs	.782 ± .025	.841 ± .022	.891 ± .011	.512 ± .053
Like-sex, 5–9 years	.882 ± .028	.921 ± .025	.946 ± .012	.788 ± .053
Like-sex, 10–16 years	.865 ± .027	.842 ± .036	.865 ± .022	.568 ± .083
All like-sex pairs	.867 ± .020	.908 ± .017	.925 ± .009	.654 ± .053
Girl-girl pairs, 5–9 years	.915 ± .026	.709 ± .112	.965 ± .009	.913 ± .030
Girl-girl pairs, 10–16 years	.814 ± .05	.896 ± .032	.919 ± .021	.521 ± .123
All girl-girl pairs	.857 ± .029	.866 ± .033	.928 ± .012	.645 ± .071
Boy-boy pairs, 5–9 years	.800 ± .078	.934 ± .049	.921 ± .041	.534 ± .161
Boy-boy pairs, 10–16 years	.890 ± .034	.747 ± .080	.895 ± .027	.715 ± .089
All boy-boy pairs	.877 ± .030	.938 ± .015	.925 ± .018	.605 ± .090
Unlike-sex pairs, 5–9 years	.774 ± .064	.519 ± .147	.753 ± .066	.681 ± .090
Unlike-sex pairs, 10–16 years	.298 ± .137	.643 ± .091	.834 ± .044	.072 ± .141
All unlike-sex pairs	.504 ± .081	.732 ± .056	.867 ± .025	.266 ± .102

TABLE III.—RESULTS OF LAUTERBACH'S STUDY OF OLDER VS. YOUNGER TWINS[1]

Trait	90–156 months	157–238 months	Difference
Intelligence quotients	.64	.73	+.09
Reading quotients	.44	.57	+.13
Arithmetic, accuracy	.59	.50	−.09
Arithmetic, speed	.59	.57	−.02
Memory for digits	.36	.34	−.02
Handwriting, quality	.49	.58	+.09
Handwriting, speed	.66	.55	−.11
Averages	.54	.55	+.01

[1] About 100 cases in each group.

Lauterbach collected data on twins, using such tests as the Terman Group test of Mental Ability, the National Intelligence Test, the Thorndike-McCall Reading Scale, and the Courtis Arithmetic Tests for testing mental ability. He also made such physical measurements as weight, height, and cephalic index and studied handedness and palm patterns. Although two tests of intelligence were used, each was given to different sets of twins, and thus he got no increase in the reliability of his results through using two tests. Tables III and IV summarize his main results.

TABLE IV.—RESULTS OF LAUTERBACH'S STUDY OF THE PHYSICAL TRAITS OF TWINS

Trait	Like-sex	Unlike-sex	Younger	Older
Height, standing	.80	.53	.61	.65
Height, sitting	.73	.59	.60	.58
Weight	.89	.50	.59	.64
Cephalic index	.67	.59	.72	.61
Averages	.78	.55	.63	.62

On statistical grounds, Lauterbach's study is open to the same criticism as Merriman's. No account is taken of the variability of the different groups compared, and many of the variations in his correlation coefficients may be due to the differences in the ranges of the groups. Further, when group tests of intelligence are used, it is desirable to give more than one test if a high degree of reliability is sought. Finally, to measure the average resemblance in traits much subject to training, the composite score from the best tests obtainable should be used, since length of testing time greatly increases the reliability of the test.

The present study was undertaken to see what results would be obtained if the defects in previous studies were eliminated. It was also thought that valuable data might be obtained if the test given to twins (similar inheritance) were also given to orphans brought up in an orphanage (similar environment). Unfortunately, for the purposes of this test, the number of orphans who had spent at least twenty-five per cent of their lives in the same orphanage was regrettably small. Only twenty-nine could be found who met the conditions laid down.

The twin subjects comprised one hundred two pairs selected at random from the public schools of Toronto and Hamilton. The orphans were twenty-nine pupils in a fraternal orphanage, a residential institution some miles north of Toronto.

The tests used were:

1. The National Intelligence Test, Scale A, Form 1.
2. The Multi-mental Scale of McCall.
3. The Stanford Achievement Test—a battery of nine sub-tests: (1) reading—paragraph meaning; (2) reading—sentence meaning; (3) reading—word meaning; (4) arithmetic—computation; (5) arithmetic—reasoning; (6) nature study and science; (7) history and literature; (8) language usage; (9) spelling.
4. The British Columbia Test in the Fundamentals of Arithmetic.
5. The Morrison-McCall Spelling Scale.

Thirteen tests in all were therefore given—the best that could be chosen. In the combined opinion of Otis, Trabue, Franzen, Van Wagenen, McCall and Kelley, the National Intelligence Test and the Stanford Achievement Test are superior to all others for use in the elementary schools, the N. I. T. being confined, of course, to pupils of nine years of age and upwards. The Multi-mental Scale, it will be remembered, was standardized against a very exacting criterion. The British Columbia Arithmetic Test and the Morrison-McCall were used as checks against the Stanford Achievement Test. As a measure of intelligence, the composite IQ from the National Intelligence Test and the Multi-mental Test was used; for achievement, the educational quotient based on all the achievement tests was determined; and for accomplishment, the AQ calculated from the ratio $\frac{\text{EA}}{\text{MA}}$ was found, since all the tests were given on the same day.

One difficulty concerning the treatment of twin data should be mentioned. In calculating Pearson r's from such data it is impossible to determine which should be considered as the x variable and which the y variable. They are interchangeable. Correlations are generally calculated between different traits of the same individuals, such as height and weight. But in twin data it is necessary to compute the correlation between the abilities of the members of the twin pairs in one trait, *e.g.*, intelligence. It can be shown that spurious correlations will be obtained if numbers are drawn in pairs by chance, or if the smaller is always considered as the x variable and the larger as the y variable. In fact the correlation obtained depends directly on the manner in which the x and y variables are chosen.

To overcome this difficulty, the Otis formula

$$r = 1 - \frac{1}{2}\left(\frac{\sigma d}{\sigma y}\right)^2$$

was used, where σd is the standard deviation of the differences between the scores of each twin pair for the trait measured, and σy is the standard deviation of the scores of all children in general for the same trait.

If the medical birth records of Canada had included statements about the chorion, no dubiety would be felt about the classification of twins into identical and fraternal pairs. Identical twins, developing from a single ovum, are invariably enclosed in a single chorion and usually have a single placenta. Fraternal twins develop from two separate ova and each is enclosed by a chorion. These data could not be secured, consequently, for the purposes of this study, only those twins that the examiner and the teachers of the school unanimously agreed upon as identical in physical appearance were placed in the identical class. All others were regarded as fraternal. Unlike-sex pairs are indubitably fraternal. Some like-sex pairs are identical, some fraternal. A summary of the findings from intelligence quotients derived from a composite mental age of the National Intelligence Test and Multi-mental is given in Table V.

TABLE V.—RESEMBLANCES OF TWINS IN GENERAL INTELLIGENCE

Group	Number of pairs	Raw r	r for constant age	Standard deviation σ	Standard error of $\sigma\sqrt{1-r^2}$	Mean difference in IQ's
All twin pairs.....	102	.76	.75 ± .029	13.5	8.92	9.65
Unlike-sex pairs.....	26	.62	.59 ± .086	12.9	10.40	12.00
Like-sex pairs.....	76	.83	.82 ± .025	13.6	7.79	8.50
Fraternal pairs.....	57	.72	.70 ± .045	12.6	9.03	11.74
Identical pairs.....	45	.91	.90 ± .019	14.3	6.23	6.23

In Table V the effect of age on the raw coefficient of correlation has been eliminated by calculating the correlation between the chronological age and the intelligence quotient. The standard error of estimate takes into account both the correlation and the standard deviation. Kelley calls $\sqrt{1-r^2}$ the coefficient of alienation. It may vary from 0 to 1, zero representing perfect correlation and 1 none at all. It is practically the inverse of the Pearson r and is calculated directly from it. To obtain the standard error of estimate this coefficient of alienation is simply multiplied by the standard deviation

(σ) of the group under consideration. The smaller this standard error of estimate, the greater is the resemblance among the individuals comprising the group. The last column gives the mean difference of the intelligence quotients of the various pairs.

Whatever criterion of resemblance is used for the groups, the order in decreasing magnitude is—(1) identical pairs, (2) like-sex pairs, (3) all twin pairs, (4) fraternal pairs, and (5) unlike-sex pairs. The coefficient for all twin pairs compared with those found by other investigators is given below:

$r = .75 \pm .029$ (present study)
$r = .78$ (Merriman's Stanford-Binet)
$r = .67$ (Lauterbach)

If the effect of age is eliminated from Merriman's raw r, it becomes .76.

The results for like-sex pairs and unlike-sex pairs are confirmed by Merriman's and Lauterbach's data. Thus:

Like-sex pairs (present study), $r = .82$
Like-sex pairs (Merriman), $r = .87$
Like-sex pairs (Lauterbach), $r = .77$
Unlike-sex pairs (present study), $r = .59$
Unlike-sex pairs (Merriman), $r = .50$
Unlike-sex pairs (Lauterbach), $r = .56$

The higher correlations for like-sex pairs is due to the inclusion of a number of uni-ovular, or identical, twins. The correlations for unlike-sex pairs approach those found for siblings by Pearson (.52), Miss Rensch (.49), Madsen (.63) and other workers. Unlike-sex twins may thus be regarded as siblings born at the same time.

The coefficient of correlation of .90 for the identical twins is practically the same as that which Newman obtained for the physical traits of the uni-ovular quadruplets of the nine-banded armadillo, or as great as that found between the right and left halves of a single individual.

The relative effects of heredity and environment may be judged in a number of ways from the data at command.

1. Younger twin data may be compared with data from older twins.

2. The resemblances of all twin pairs in native traits (as judged by an intelliegence test) may be compared with acquired traits (as judged by achievement tests).

3. The results obtained by the orphans living in a uniform environment may be compared with data from children living in a non-uniform environment.

Table VI presents the facts for younger and older twins.

TABLE VI.—SUMMARY TABLE OF DATA FROM YOUNGER TWINS (FORTY-FIVE PAIRS) COMPARED WITH DATA FROM OLDER TWINS (FIFTY PAIRS)

Trait	Twins, 8–11 years			Twins 12–15 years		
	Raw r	r for constant age	Standard error of estimate	Raw r	r for constant age	Standard error of estimate
General Intelligence (IQ)	.73	.71 ± .047	8.09	.78	.77 ± .038	9.16
Stanford Achievement (EQ)	.73	.64 ± .060	8.13	.90	.87 ± .023	5.35
Stanford Achievement (AQ)	.82	.82 ± .033	3.95	.72	.72 ± .046	5.10
Arithmetic	.94	.89 ± .022	2.70	.85	.73 ± .045	4.00
Spelling	.89	.85 ± .029	4.18	.89	.85 ± .026	4.41
Average	...	.782	5.41	...	.788	5.60

In the cases of the Stanford Achievement AQ's, arithmetic scores and spelling scores, it is seen that younger twins are more alike than older twins whether measured by the coefficient of correlation or the standard error of estimate. However, the Stanford Achievement EQ's favor the older twins. The results from the IQ's are conflicting—the coefficient of correlation favoring the older, and the standard error of estimate the younger twins. By finding the probable error of the difference between the two coefficients, we can discover whether or not the difference between the two coefficients (.71 and .77) is a real one.

Thus

$$\begin{aligned} PE_{\text{(diff. between two coefficients)}} &= \sqrt{PE^2_{\text{(one coeff.)}} + PE^2_{\text{(other coeff.)}}} \\ &= \sqrt{.047^2 + .038^2} \\ &= .0604 \end{aligned}$$

We see, therefore, that the difference between the two coefficients, .06, is just equal to the probable error of the difference between them. This means that there is a one-to-one chance that the difference found between the coefficients for the IQ's of younger and older twins is entirely accidental.

The evidence on the whole seems to point to the fact that older and younger twins are about alike, with the evidence leaning slightly to the side of the younger twins. If this be the case, then heredity would seem to be a more potent factor than environment, since if it were not, then older twins should be more alike than younger twins. And they are not.

Comparing the resemblances of twins in native and acquired traits we get Table VII.

TABLE VII.—RESEMBLANCES OF ALL TWIN PAIRS IN NATIVE AND ACQUIRED TRAITS

1. EQ's Stanford Achievement	$r = .76 \pm .029$ (94 pairs)
IQ's (general intelligence)	$r = .75 \pm .029$ (102 pairs)
Difference	$= .01 \pm .041$
2. AQ's Stanford Achievement	$r = .83 \pm .021$ (94 pairs)
IQ's (general intelligence)	$r = .75 \pm .029$ (102 pairs)
Difference	$= .08 \pm .036$
3. Arithmetic scores	$r = .78 \pm .028$ (88 pairs)
IQ's (general intelligence)	$r = .75 \pm .029$ (102 pairs)
Difference	$= .03 \pm .040$
4. Spelling scores	$r = .85 \pm .019$ (92 pairs)
IQ's (general intelligence)	$r = .75 \pm .029$ (102 pairs)
Difference	$= .10 \pm .035$

The differences are small and the probable error of the difference shows them to be insignificant; the chances are less than one to one that they are real differences at all, at least in the cases of EQ's and Arithmetic Scores. For AQ's and for spelling the chances are slightly greater. We may, therefore, conclude that twins are no more alike in those traits much subject to training (reading, arithmetic, spelling, literature, nature study, and history) than they are in general intelligence, a trait that is supposedly not directly affected by schooling.

In the treatment of the data from the twenty-nine orphans two methods of pairing were used. The first was to pair an orphan with another who was nearest to him in chronological age. The second was to pair the orphans with each other at random. Table VIII A and B gives the results.

The above figures show that not only is there no resemblance when orphans are paired at random for intellectual traits, but there is a fair degree of inverse correlation.

TABLE VIII

A. Orphans Paired at Random (Fifteen Pairs)

Trait	Raw r	Correlation between age and trait	r for constant age
IQ's	−.49	−.18	−.54
EQ's	−.19	−.62	−.79
AQ's	−.54	−.38	−.79
Arithmetic	.14	.58	−.30
Spelling	.08	.61	−.46
Average	.....	.....	−.58

B. Orphans Paired to Nearest Age (Fifteen Pairs)

IQ's	.16	−.18	.13
EQ's	.34	−.62	−.07
AQ's	.54	−.38	.47
Arithmetic	.46	.58	.18
Spelling	.59	.61	.35
Average	...	.....	.24

When paired for nearest age, a low degree of resemblance is shown (.24). While too much weight cannot be attached to figures from twenty-nine cases, they certainly do not substantiate the belief that unrelated children, when reared in the same environment, will show any greater resemblances when paired at random, than will unrelated children who have been reared apart. Environment seems impotent to modify greatly the original natures of children.

The IQ's of the two hundred four twins ranged from sixty-three to one hundred forty-six. The median IQ was 96.8, the mean 97.2 and the SD 13.5. The twins, therefore, were slightly below the general level of the population in intelligence. This peculiar fact has been observed by other workers. Merriman reported the median IQ of his twins as ninety-seven; Lauterbach's was ninety-five. The inferiority is slight, but it seems to be present in all data so far published.

For the orphans, the range of IQ's was from seventy to one hundred twenty-nine. The median IQ was 93.0, the mean 92.6 and the SD 9.15. Orphans were about seven points below the average of the population in intelligence. This was to be expected and confirms the evidence from other studies.

In conclusion, we present in Table IX a summary of resemblances among groups showing different degrees of genetic relationship. The

first four and last are from the present study; the others from the researches of other workers.

TABLE IX.—COEFFICIENTS OF CORRELATION FOR INTELLIGENCE AMONG GROUPS EXHIBITING DIFFERENT DEGREES OF GENETIC RELATIONSHIP

Group	*r*
Physically identical twins	.90
Like-sex twins	.82
Fraternal twins	.70
Unlike-sex twins	.59
Siblings	.50
Parent-child	.31
Cousins	.27
Grandparent-grandchild	.16
Unrelated children	.00
Orphans	.00

The principle of homology maintains that closeness of resemblance is in proportion to closeness of kinship. So far as intelligence is concerned, the principle is supported by the figures presented above.

SUMMARY

1. There is no significant difference in the amount of resemblance in mental traits between younger and older twins.

2. Twins are no more alike in those traits upon which the school has concentrated its training than in general intelligence.

3. Hence, considering 1 and 2, environment is inadequate to account for the mental resemblances of twins.

4. Like-sex pairs of twins show a greater degree of resemblance in intelligence than unlike-sex pairs.

5. Unlike-sex pairs of twins have approximately the same degree of resemblance in intelligence as siblings.

6. There are two distinct types of twins because:

(*a*) The like-sex group which must partly consist of a number of uni-ovular, or identical pairs, shows a higher degree of mental resemblance than the unlike-sex group.

(*b*) Physically identical pairs show a higher degree of resemblance than fraternal pairs.

(*c*) The degree of resemblance of siblings in mental traits is nearer to that of unlike-sex pairs than to that of the like-sex pairs. This

bears out the contention that unlike-sex pairs are, from the genetic standpoint, really siblings that are born at the same time.

(*d*) Members of fraternal pairs of twins show, on the whole, greater diversity in school grades than members of physically identical pairs. This latter group is probably composed largely of uni-ovular twins.

7. Orphan children who have been reared together for a considerable portion of their lives, are no more alike than unrelated children paired at random, either in general intelligence or other intellectual traits.

8. Twins as a group are very slightly below (two to three per cent) the average of the population in general intelligence, but show about the same degree of variability as unselected children.

9. Orphan children are about seven per cent below the average intelligence of unselected children.

10. The amount of resemblance in general intelligence varies from $r = 0$ for unrelated individuals to a maximum of $r = .90$ for physically identical twins. Intermediate values are found in accordance with the genetic relationship of the individuals. Therefore, there is an increasing degree of resemblance in general intelligence among human beings with an increasing degree of blood relationship among them. *Ergo*, general intelligence is an inherited trait.

Bibliography

1. Arey, L. B.: "Direct Proof of the Monozygotic Origin of Twins. *Anat. Rec.*, Vol. XXIII, pp. 245–251.
2. Ballard, P. B.: "Mental Tests." Toronto, Hodder and Stoughton, 1920
3. Binet, A.: The Development of Intelligence in Children. Publications of the Vineland Training School, New Jersey, 1916.
4. Binet, A.: L'Intelligence des Imbeciles. *L'Annee Psychologique*, 1909.
5. Burt, C.: Mental and Scholastic Tests. London: P. S. King and Son, 1922.
6. Cattell, J. McK.: A Statistical Study of American Men of Science: III. The Distribution of American Men of Science. *Science*, New Series, Vol. XXIV, pp. 732–742.
7. Cobb, M. V.: Evidence Bearing on the Origin of Twins from Single Ova. *Science*, New Series, Vol. LXVI, pp. 501–502.
8. Conklin, E. G.: "Heredity and Environment." Princeton University Press, 3rd, 1920.
9. Davenport, et al.: Twins. *Journal of Heredity*, Vol. X, Dec., 1919, p. 9.
10. Dexter, E.: On Family Resemblance beyond the First Degree of Relationship. *School and Society*, April 26, 1924, pp. 501–502.
11. Dugdale, R. L.: *The Jukes.* Thirty-third Annual Report of the Prison Association of New York, 1876.

12. Elderton, E. M.: On the Measure of Resemblance of First Cousins. *Eugenics Laboratory Memoirs*, Vol. IV, pp. 1–53. London: Dulau and Co., 1907.
13. Elderton, E. M.: A Summary of the Present Position with Regard to the Inheritance of Intelligence. *Biometrika*, 1923, Vol. XIV, pp. 378–408.
14. Estabrook, A. H.: "The Jukes in 1915." Washington, Carnegie Institution, 1916, pp. 85.
15. Fisher, R. A.: The Resemblance between Twins: a Statistical Examination of Lauterbach's Measurements. *Genetics*, Vol. X, No. 6, Nov., 1925, pp. 569–579.
16. Galton, F.: "Heredity Genius: an Inquiry into Its Laws and Consequences." London: Macmillan and Co., 1914 (First Edition, 1869).
17. Galton, F., and Schuster, E.: "Noteworthy Families." London: Eugenics Record Office, 1906.
18. Galton, F.: "English Men of Science." New York: Appleton and Co., 1874.
19. Galton, F.: "Inquiries into Human Faculty and Its Development." New York: E. P. Dutton and Co., 1908 (First Edition, 1881).
20. Galton, F.: "Natural Inheritance." London: Macmillan and Co., 1889 (First Edition, 1867).
21. Gesell, A.: Mental and Physical Correlation in Twins. *Science Monthly*, 1922, Vol. XIV, pp. 305–331, 415–418.
22. Goddard, H. H.: "The Kallikak Family." New York: The Macmillan Co., 1912.
23. Gordon, K.: Psychological Tests of Orphan Children. *Journal of Delinquency*, Vol. XVIII, Jan., 1919, pp. 46–55.
24. Gordon, K.: The Influence of Heredity on Mental Ability. Report of the Children's Department, State Board of Control of California, 1918–1920.
25. Hart, B. and Spearman C.: General Ability: Its Existence and Nature. *British Journal of Psychology*, Vol. V, pp. 51–84.
26. Lauterbach, C. E.: Studies in Twin Resemblance. *Genetics*, Vol. X, No. 6, Nov., 1925, pp. 525–568.
27. Madsen, I. N.: Some Results with the Stanford Revision of the Binet-Simon Tests. *School and Society*, May 10, 1924, p. 559.
28. Merriman, C.: The Intellectual Resemblance of Twins. *Psychological Monographs*, Vol. XXXIII, No. 5, 1924.
29. Newman, H. H.: "The Biology of Twins." Chicago: University of Chicago Press, 1915.
30. Newman, H. H.: "The Physiology of Twinning." Chicago: University of Chicago Press, 1923.
31. Newman, H. H.: Evolution, Genetics and Eugenics. Chicago: University of Chicago Press, Second Edition, 1925.
32. Pearson, K.: On the Laws of Inheritance in Man. *Biometrika*, Vol. III, pp. 131–190.
33. Pearson, K.: On the Relationship of Intelligence to Size and Shape of Head and to Other Physical and Mental Characters. *Biometrika*, Vol. V, pp 105–146.
34. Pearson, K.: The Inheritance of Psychical Characters. *Biometrika*, Vol. XII, pp. 367–372.

35. Popenoe, P., and Johnson, R. H.: "Applied Eugenics." New York: The Macmillan Co., 1918.
36. Sano, F.: A Convolutional Pattern of the Brains of Twins: a Study on Hereditary Resemblance in the Furrows of the Cerebral Hemispheres. *Philosophical Transactions Royal Society*, London, B208, pp. 37–61.
37. Schuster, E., and Elderton, E.: The Inheritance of Ability. Eugenics Laboratory Memoirs, 1907.
38. Shen, E.: The Intellectual Resemblance of Twins. *School and Society*, Vol. XXI, No. 542, May, 1925.
39. Spearman, C.: "The Nature of Intelligence and Principles of Cognition." London: The Macmillan Co., 1923.
40. Starch, D.: The Similarity of Brothers and Sisters in Mental Traits. *Psychological Review*, Vol. XXIV, 1917, pp. 235–238.
41. Stern, W.: "Psychological Methods of Measuring Intelligence." London: Warwick and York, 1913.
42. Terman, L.: "The Measurement of Intelligence." Boston: Houghton Mifflin Co., 1916.
43. Thorndike, E. L.: Measurements of Twins. Archives of Philosophy, Psychology and Scientific Methods, No. I, 1905.
44. Thorndike, E L.: "Educational Psychology," Vol. III. New York, Teachers College, Columbia University, 1914.
45. Wiggam, A. E.: "The Fruit of the Family Tree." Robbs-Merrill Co., 1924.
46. Woods, F. A.: "Mental and Moral Heredity in Royalty." New York: Henry Holt and Co., 1906.

THE CONDITIONED REFLEX AND THE LAWS OF LEARNING

GEORGE HUMPHREY

Queen's University, Kingston, Canada

In an interesting and suggestive paper, published in this Journal,[1] Professor Percival Symonds has given twenty-three descriptions, or laws of learning, based on Pavlov's investigations into the conditioned reflex. The data on the Conditioned Reflex were taken from the extremely condensed account of C. Lovatt Evans in his "*Recent Advances in Physiology*," an account, which is likely at times because of its simplicity, to lead to misconception. There are now available two additional sources[2] which enable certain corrections to be made. In addition, there are certain theoretical qualifications which should, perhaps, precede the application of Pavlov's results to teaching practice. It seems therefore desirable to add certain cautions to Professor Symonds' account.

In general, it may be said that there is no *a priori* reason why Pavlov's results, obtained under highly controlled laboratory conditions, should be directly applicable to the problems of the nursery or classroom. There is need, as Professor Symonds rightly insists,[3] of experimental verification. In particular, one fundamental point emerges. Pavlov worked with isolated stimuli, tones, lights, scratch stimuli, etc. But there is no reason to believe that the ordinary situations of everyday life may always be analyzed into such isolated stimuli, or that such isolated stimuli when transferred into everyday life will function as they do in the artificial conditions of the laboratory. Often, Pavlov's results do seem to apply directly to extra-laboratory conditions; but there are many cases where an attempt at such application would appear to be misleading.

As an example, the writer may perhaps be pardoned for instancing certain results obtained in the Queen's laboratory.[4] Subjects were

---

[1] Symonds, Percival M.: Laws of Learning. *Journal of Educational Psychology*, Vol. XVIII, No. 6, Sept., 1927.

[2] *Viz.*, a German translation of Pavlov's "Twenty Years' Objective Study" (München, Verlag von J. F. Bergmann, 1926); and Pavlov, J. P.: "Conditioned Reflexes." Oxford, 1927.

[3] *Op. cit.*, p. 405.

[4] The Effect of Sequences of Indifferent Stimuli on a Response of the Conditioned Reflex Type. These experiments were described in the *Journal of Abnormal Psychology*, Vol. XXII, No. 2, 1927, pp. 194–212.

conditioned to respond to a certain tone produced by striking a metal cylinder with a small wooden hammer. Other tones had been "discriminated out," so that the best of the subjects responded to one tone only. Finally, the subject had been so well trained that his response followed as a matter of course from his note in isolation. But when the same tone was given as one of a series of tones, he hardly ever responded at first. One subject, for example, was trained to respond to note number 7 on the instrument. Our records show that on one occasion he responded thirty-three times to note 7 played in isolation, but when the sequence 5, 7, 9, 12 (a common chord) was played *seriatim* as an *arpeggio*, no response took place, although this series, containing his note each time, was repeated twelve times altogether. Here a well drilled conditioned response did not function when the conditions of experiment were slightly altered. From the psychological point of view, the series 5, 7, 9, 12 could not be analyzed into the constituent isolated notes 5 and 7 and 9 and 12. We do not, as yet, know the principles on which everyday situations should be psychologically analyzed. Until we do, we cannot be certain whether a response trained by the Pavlov method will or will not appear. Of course, the above result may, when it occurs, be explained by Professor Symond's headings 17 and 17a, which speak of the weaking effect of the previous presentation of additional stimuli. But it should be pointed out that the conditioned response did not always thus fail to appear when the stimulus was presented as part of a series,[1] though this was the rule with the arpeggios. The thing was highly irregular, and we felt that we did not understand the conditions. Throughout our experiment, which extended over a period of fifteen months, we were constantly struck by the capricious nature of the conditioned response. It changed from day to day, sometimes it would disappear for a whole experimental period, though after very prolonged training it attained a remarkable constancy in certain subjects. Some subjects, in the early stages of the experiment, failed to develop it at all; one subject after more than eight hundred combined stimulations. This was undoubtedly due to a faulty technique which was later corrected. But similar results are reported by other experimenters, and they show that the direct transplantation of Pavlov's rule to the practical problems of learning is attended by very insistent dangers.

In addition to these general considerations, there are also certain specific qualifications that should be made to the rules deducible from

[1] See the original paper, p. 202 and elsewhere for instances.

Lovatt Evans' or any of the condensed accounts of Pavlov's work. The rules may be taken *seriatim*.

1. *Simultaneous Stimulation.*—Stimulation of a receptor organ occurring simultaneously with a reaction of an effector organ leads to the formation of a new stimulus response bond. This appears to be misleading. The secondary stimulus (light or bell) is regularly given first, and is simultaneous with the primary stimulus only for a time. Thus as far back as 1906 Pavlov[1] describes the formation of a response to scratching. "The first fifteen seconds, scratching alone takes place; prolong it to the end of the minute, but simultaneously pour acid in the dog's mouth. Finally, the conditioned reflex has appeared." In the same paper Pavlov speaks of the disappearance of the response when the stimulus "ceases to be a correct signal." In fact, in his lectures in 1924, published in English under the title "Conditioned Reflexes,"[2] he contrasts the inborn, unconditioned reflexes with the conditioned, *signal* reflexes. A signal is obviously not primarily simultaneous with that which it signalizes. Its chief use is anticipatory. In fact, the following directions are given by Podkopaew, one of Pavlov's co-workers, for the establishment of "strictly simultaneous reflexes:" "One or the other conditioned stimulus is allowed to operate and one to two seconds after the beginning of its operation an unconditioned stimulus is added."[3] Podkopaew's little manual, which is a practical guide for the laboratory worker, has a short accompanying notice by Pavlov, who refers to the author as "a worker in this field for many years," so that the account may be taken as official. It is all the more surprising, therefore, to find the reports of Pavlov's work so misleading on the subject of the coincidence of the two stimuli.[4] It may be noted that the writer in common with some other experimentors who have worked on human beings, also found it better to present the secondary stimulus first—to make a signal—response, in Pavlov's terminology—although this is not essential. The point here to be noted is that in the most fundamental work done on the conditioned response, the two stimuli were not "simultaneous"

---

[1] Pavlov, J. P.: "Die Höchste Nerventatigkeit (das Verhalten) von Tieren." P. 56.

[2] For reference see Note 1, Chapters I and II *passim*.

[3] Podkopaew, N. A.: "Die Methodik der Erforschung der bedingten Reflexe." München, 1926.

[4] *e.g.*, Dontchef-Dezeuze in her book "L'Image et les Réflexes Conditionnels," (Paris, Alcan, 1914), has, as far as I can discover, no hint that the one stimulus precedes the other.

in the sense that they began and ended together. "It is," says Pavlov himself, "necessary that the conditioned stimulus should begin to operate before the unconditioned stimulus comes into action."[1]

Suppose however that we subject the organism simultaneously to stimuli which are biologically adequate to produce salivary reactions and flight reactions respectively. Will the upshot be that the flight stimulus will cause the animal to salivate, or will the salivary stimulus cause him to run away? One of Pavlov's workers[2] found that stimuli could be arranged in a hierarchy, according to physiological strength and weakness, reminding one of Sherrington's conception of prepotency. Thus a powerful electric current was converted into an alimentary conditioned stimulus, "so that its application to the skin did not evoke the slightest defence reaction."[3] On the other hand, a current applied to skin overlying bone could not be thus "conditioned." So that, again sheer simultaneity is not enough. Precedence in time and in biological importance are both to be considered.

The law of repetition, Professor Symond's second generalization, seems to require comment. According to Podkopaew,[4] "indications of the appearance of the conditioned salivary reaction show themselves after the third to the fifth time, stabilization does not occur until ten or fifteen strengthening applications. The second, third, etc. C. R. are much quicker, and are often perfected at the second repetition." Now many of the responses with which we work in school would seem to belong to the latter category, and Professor Symonds points out that in human linguistic learning a single repetition alone is sometimes sufficient. According, however, to Herrich, "the evidence is clear that this reorganization of the conduction paths . . . is usually very gradually effected"[5] and, in so saying, he takes issue with Lashley. The possibility of the formation of conditioned responses at a single repetition is mentioned also by Burnham,[6] who was thinking of learning under emotion, and had in mind the "traumatic" cases of the Freudian school. Pavlov speaks of a case observed in his labor-

---

[1] *Loc. cit.*, p. 27.

[2] "Conditioned Reflexes." P. 31.

[3] *Ibid.*, p. 29.

[4] *Op. cit.*, p. 35.

[5] C. J. Herrick: "Brains of Rats and Men." University of Chicago Press, 1926, p. 65.

[6] Burnham, W. H.: "The Normal Mind." Appleton, 1924, Chap. XIX. But *cf.* p. 65, where he heads a paragraph "Repetition necessary."

atory,[1] and Doutchef-DeZeuze also mentions the same possibility for "natural" conditioned responses.[2] It would perhaps be fair to say that the work on the conditioned response has pointed the way to a possible reformulation of the law of use.[3]

Professor Symond's third law, which states that the conditioned stimulus cannot come after the unconditioned one, also deserves comment. Beritoff, another of Pavlov's co-workers and a neurologist of note, claims to have obtained with pigeons results that directly contradict the view of Pavlov's school that such reversed conditioned reflexes are impossible. Whether or not backward conditioned responses are possible in the normal human being, experiment alone must decide.[4] It is certain that they would, at least, be much more difficult to establish, so that Professor Symond's practical statement of the principle is probably justified.

Rule 11, that "a conditioned reflex established to any given stimulus does not lead to formation of other conditioned reflexes," I think, hardly bear the interpretation put upon it. Professor Symonds claims that it "denies categorically the possibility of transfer." But when a conditioned response is formed, at first this response will follow the presentation of a large number of stimuli other than the original one. Thus, during our training work, the students would occasionally amuse themselves by shouting from the operator's table, when the subject would often pull his hand up as if he had heard the note or received a shock. Nor can it be said that a "tenous, highly aerated aspect" of the situation, to use Dr. Symond's phrase, was here the conditioned stimulus, for we used a very definite single tone. The fact seems to be that, *at a certain stage in the training*, the conditioned response automatically transfers itself to other stimuli which have never been associated with the unconditioned stimulus. These "accessory reflexes," to use Pavlov's term, have to be stamped out by repeatedly reinforcing the "active" stimulus while leaving the inactive one unreinforced. Pavlov refers to their biological use, in that the natural stimuli are as a general rule variable; it is clearly to the animal's

---

[1] *Ibid.*, p. 27.

[2] *Op. cit.*, p. 16.

[3] *Cf.* Cason, Hulsey: Criticisms of the Law of Exercise and Effect. *Psychological Review*, Vol. XXXI, No. 5, 1924.

[4] Beritoff, J.: Individuell erworbene Tätigkeit des zentral Nervensystems bei Tauben. *Pflugers Archiv.*, No. 213, 1926. The possibility of "Backward Association" in human beings has been investigated by Cason and others. *Cf.* Cason, Hulsey, in the *Journal of Experimental Psychology*, Vol. IV, No. 3, 1926.

advantage to react to the "hostile sound of a beast of prey . . . independently of variations in pitch, intensity, and timbre." This phenomenon of generalization I am obliged to interpret as a clear case of transfer. The animal has learned to respond in a certain way to one stimulus; during training he *invariably* attaches that response to a set of other stimuli which have not been concerned in the original learning process; what is more, he does so until responses to these stimuli have been deliberately trained out. As to the final specificity of the response, if this is definitely encouraged, there is no doubt. When this specificity has been definitely trained, then there can be no transfer in Professor Symond's sense. The fact of generalization is mentioned in the article of Professor Symonds under consideration (p. 411, rule 16), and the case in question is referred to "irradiation in the same sense organ." It is to be noted that Pavlov[1] now refers the phenomenon to irradiation in the cerebral cortex. That is to say, this transfer, if I am correct in so terming it, is due to a general property of the brain, not to a mere sensory smudging.

Laws 13, 13a, 14, 15 and 16 concern the phenomenon known as "extinction," which is Pavlov's name for the degeneration of the response which follows repetition of the conditioned stimulus without the unconditioned stimulus. This is one of the most puzzling of the properties of the conditioned response, and it is named by Professor Symonds the law of fatigue. There are several reasons why it is perhaps assuming too much to give to the phenomenon of extinction the name of fatigue, although it is so called by Watson. We have duplicated the phenomenon many times with human beings, and have observed that the response may reappear after a very brief interval such as is necessary to change the kymograph lever. Reestablishment of the response after so brief an interval is hardly consistent with the use of the term fatigue, at least as this term is ordinarily understood. Moreover, Pavlov claims that the phenomenon is caused by a specific form of inhibition,[2] which he calls internal inhibition. Now inhibition is for Pavlov an active checking process, which can, he claims, be itself "conditioned." He advances "as a strong probability" the view that "internal" and "external" inhibition are "fundamentally the same,"[3] and it cannot be claimed that interference exerted by an extraneous stimulus could be called a fatigue phenomenon. In fact during

[1] "Conditioned Reflexes." P. 186.

[2] *Cf.* law 17c, Symonds, *loc. cit.*, p. 411.

[3] "Conditioned Reflexes." P. 388.

conditions such as pregnancy and inanition, which would seem to favor fatigue as ordinarily understood, the inhibitory activities are the first to be weakened, and it is hard to establish fine discriminations—dependent, again, according to theory, on inhibitions—during such states. Inhibition, whether "internal" or "external" is not for Pavlov fatigue, although, to be sure, he identifies the former with sleep.

Law 16, that "extinction of a conditioned reflex has no effect on the strength of other conditioned reflexes," offers a striking example of the disagreement of authorities, with danger to those of us who may attempt to use their findings. Lovatt Evans makes this statement, relying on the authority of Dr. G. V. Anrep, yet another coworker of Pavlov. Pavlov makes an exactly opposite statement,[1] distinguishing between primary and secondary extinction, the latter being that form which is caused in one conditioned reflex by the extinction of another. Sometimes, he says, even the unconditioned reflexes may thus be involved.

It is to be understood that these criticisms of Dr. Symond's paper are in no way intended as anything more than a supplement, which has been made possible mainly by the long awaited translation of Pavlov's works. I feel that, in fairness to the method and those who have worked on it, such a supplement should now be added.[2]

---

[1] "Conditioned Reflexes." P. 54.

[2] Pavlov himself has in "Conditioned Reflexes" a chapter on the human applications of his work. An air of extreme caution is noticeable throughout. He several times uses such phrases as "the incalculably complex symptoms" in man, and actually states that "at the present stage of our work no detailed application of its results to man is yet permissible." This is the reticence of the man who knows the difficulties and complexities and it does not, of course, preclude the use of the method in direct experimental attack on problems of human learning.

EYE PREFERENCE TENDENCIES[1]

A. M. AND MARGUERITE A. SNYDER

University of Pittsburgh

There seems to be very little reliable data in regard to eye preference in visual perception. Among optometrists who furnish data or express an opinion, there is a rather current belief that those who are right-handed are also right-eyed, *i.e.*, have a right eye preference, and *vice versa*.

Unilateral hand tendencies and hand preferences have long been known, but to what extent there is a like tendency in eye function, little specific data is available. The bilateral neural arrangement in the basal ganglia for the control of the various photic reflexes, the distribution of the corresponding retinal areas, and the dual cortical visual arrangement, function and loss of retinal function, all seem apriorily to indicate bilateral visual function. These neurological visual components seem to suggest no innate superiority of either eye. But in actual practice, *i.e.*, visual perception, eye preferences are clearly manifested. Most people *do* use one eye more than the other.

In order to obtain more reliable data in regard to monocular and ambiocular tendencies, the authors' Test for Eye Preference was given as a group test to 410 students (129 were in high school, 135 in normal school and 146 were Freshmen and Sophomores at the University of Pittsburgh). The test is based on the projection of the visual image. It consists in fixating a designated point on the test card and, without moving the eyes, bringing the top of a pencil or the point of the finger in line with the line of sight about midway between the card and the eyes. As either left or right eye is closed the results are noted and interpreted according to the information contained on the card.[2]

In this brief survey of high school, normal school, and university students it was found that only about 15 per cent have no eye preference, *i.e.*, are ambiocular. As is shown in the accompanying table, on the average 64 per cent have a right monocular preference and 21

---

[1] The data were collected and compiled by Mildred Kerr, Coraopolis High School, Orville C. Wrigley, Slippery Rock Normal School and John T. Seaton, University of Pittsburgh.

[1] Copies of these tests can be obtained from the authors at a nominal price.

per cent a left monocular preference which makes a total of 85 per cent who have a distinct eye preference.

TABLE SHOWING THE PER CENT OF EYE PREFERENCE TENDENCIES

	Number of cases	Left monocular	Right monocular	Total monocular	Ambiocular
High school.....	129	21	66	87	13
Normal school.....	135	20	65	85	15
University.....	146	23	61	84	16
Average.....	...	21	64	85	15

Since the ages of these students varied from about an average of 15 years in high school to about 20 years at the university, the results seem to show that ambiocular tendencies increase with age and monocular tendencies decrease, the former varying from 13 to 16 per cent and the latter from 87 to 84 per cent. This probably means that in a very large per cent of cases the preference for either the right or left eye is implicitly acquired and not natively rooted. Although there are great differences in right and left monocular adjustments and visual acuity, they do not necessarily accord with eye preferences.

Furthermore, the same discrepancy is found between eye preference and hand preference. To be right handed does not necessarily mean a right eye preference, and *vice versa*. Although in most cases the one tendency is in accord with the other, the large per cent of exceptions seems to prove the vicariousness of eye preference tendencies.[1] It seems to indicate that the preference for one eye, either right or left, is cortically rooted and not infra-cortical as are the various monocular and binocular adjustments.

The pedagogical significance of this is further emphasized by one specific case of cortical blindness of the left eye of a student in the sophomore class of the University. In our first interview, he said he was completely blind in his left eye, although in less than one month's daily practice he could see as well with the left eye as with the right. The reason for this case of cortical blindness was due to the malfunctioning of the external and internal recti muscles of his left eye, which precluded the normal coordination of the two eyes.

[1] Specific data on the relationship between eye preference and hand preference and visual acuity will be submitted in a later report.

In summary, these preliminary results seem to show:

1. That more than three-fourths of the students between the ages of 15 and 22 years have a distinct eye preference.

2. That eye preference is an implicitly acquired visual habit and that the degree of monocular and ambiocular tendency varies with age. Both tendencies seem subject to a high degree of modification.

3. That eye preference is not necessarily caused by the inferiority or superiority of one eye. In many cases the preferred eye is the weaker eye. As such in at least some cases the greater weakness of the preferred eye is probably a resultant of eye preference and not the cause.

4. Although there seems to be a high correlation between eye preference and hand preference, the one tendency is not necessarily a reliable index of the other.

5. There is a distinct need of concentrated efforts in obtaining reliable data on the origin, causes, and individual differences in eye, ear, and hand preference tendencies and their variations and degree of modifiability from infancy to adulthood.

NEW PUBLICATIONS IN EDUCATIONAL PSYCHOLOGY AND RELATED FIELDS OF EDUCATION

CONDUCTED BY FRANCES M. FOSTER

Devices for Word Mastery

New Methods in Primary Reading, by Arthur I. Gates. New York: Bureau of Publications, Teachers College, Columbia University, 1928.

Approximately ten years of investigation in the field of reading, have led Mr. Gates to two convictions. One was that "most reading difficulties have their genesis in the work of the first year." The other was that "most of the deficiencies may be prevented by improved methods of teaching." With these two premises, then, experiments were made to evaluate the prevalent methods of teaching reading and to set them against a new improved scheme. The results of these investigations and observations form the basis for his latest book "*New Methods in Primary Reading*." The last three chapters set forth the new "method."

The author's conviction might have been that word mastery is the primary requisite for beginning reading. All of the experiments were set up in terms of that criterion. First, an analysis was made of the ways in which children recognize words. The conclusion that "no single method appears to be sufficient for all the demands of word perception . . . It is possible that not all the devices, but only a select few, are needed," is very significant. It emphasizes, in my judgment, the need for individual techniques, and also, the necessity for discovering which ones are most effective and economical for each child.

Then an analysis of the "natural" or "incidental" method, in which there is very little systematic instruction. The author concludes that these pupils "frequently fail to develop satisfactory techniques of word analysis and recognition; that the method is really "an unabridged trial and error or trial and accidental success procedure," and that the development of the basal perceptive skills should

be a definite objective of teaching. This conclusion is also important in that it reveals objectively the inadequacy of leaving to chance the development of such a complex process as reading.

But the most significant contribution made by Mr. Gates, in my judgment, is his careful study of the phonetic possibilities in the English language and his questioning of the use of phonetic methods. Other students in this field have done one of two things. Either they have accepted the traditional belief in the necessity of phonetic training for word mastery, or else they have compared it with procedures which did not include such training. The first group was concerned with building up new systems, no two agreeing as to what a phonetic word is. The second group said "Phonics or no phonics?" Mr. Gates says rather, "Word mastery by phonetic training or word mastery by another 'method'?" His experiments indicate that word mastery can be acquired without specific phonetic training.

And this emphasis leads one to consider Mr. Gates' "method" described in the last three chapters of the book. It is an organized scheme of what the author calls "intrinsic" devices. The term "intrinsic" signifies that the training occurs in actual reading situation, where meaning is stressed, and not as supplementary or extraneous drill. Accuracy of perception of word forms as wholes, of parts of words—syllables and phonograms—is brought about in each exercise at the same time that comprehension is being stressed. These exercises form a very efficient system of word recognition and word mastery devices.

If Mr. Gates had stopped at this point, set up the principles of his "intrinsic" psychology and said, "Here is what I have found to be most effective for teaching the skills," his scientific conclusions could have been accepted by teachers and used as the results of scientific study should be used. But, unfortunately, to my way of thinking, he has incorporated this scheme into an "organized course for beginning reading." It is as though a technician had learned the laws of harmonic construction in music and set about writing a musical composition to illustrate these laws.

What could be more dull than teaching the words *dog*, *cat*, *boy*, and *girl* on the first day, *run* and *play* on the second, and the words *color*, *blue*, *red*, *brown*, and *white* on the third? And then, in order to introduce phrases and sentences easily, combine these words into "*the blue boy*, *the white cat*, possibly *the brown girl*, etc."

Interestingly enough, other outcomes are obtained by this "method." But they are concomitants, developed incidental to the scheme and without "detracting from it." Some of these are *interest in school, independence* and *initiative, concentration*, and others. In short, the most fundamental outcomes of all teaching are accounted for incidentally without detracting from the acquisition of skills.

This is my issue with Mr. Gates. He has set up predominantly, as a goal, the development of word mastery, confident that all the other outcomes will appear if the skills are acquired. He has left to chance certain very vital attitudes and experiences in beginning reading in the same way that those whom he calls "opportunists" leave the skills to chance. He has not recognized the philosophy of introducing reading as a part of other activities, making those first experiences of personal significance to the child. In short, his "method" is a system of intrinsic devices for extrinsic reading situations.

LOUISE KRUEGER.

How Does Music Affect You?

The Effects of Music, a Series of Essays, edited by Max Schoen. New York: Harcourt, Brace and Company, Inc., 1927. Pp. 273.

What happens to a person when he listens to music? Here is a field of fascinating research which has until recently baffled scientific workers, especially those who like dealing in controllable variables. That it is a field which has not been without its intrepid explorers, this volume attests.

About twenty years ago, W. V. Bingham, who writes the introduction to this series of essays, working in the laboratories of Angell and Münsterburg, "sought to capture on the smoked surface of the kymographic drum those elusive changes in muscular tension which accompany the hearing of music." This early interest in the study of the effects of music prompted him to initiate in 1921, through the American Psychological Association, the competition through which many of the studies reported in this volume came into being. The prize offered was for the most meritorious research on the effects of music. The essays in this volume were, in the main, chosen from the papers submitted in this contest.

The essays include descriptions of investigations and experimental studies on the following phases of the effects of music: Types of

listeners to music; the sources and nature of musical enjoyment; the mood effects of music and related problems; how stimulating or quieting music affects heart action as measured by the Einthoven Galvanometer. Section five is given over to studies dealing with the effects of familiarity and repetition upon the hearer; a comparison of group and individual reactions to classical and jazz selections, respectively, when they are repeated a number of times in succession. Section six contains a paper on the visual, kinæsthetic, olfactory, and gustatory effects of music, and Dr. Gatewood's study on the use of similes for describing music effects.

Brief summaries of important conclusions are frequently interpolated, and in some cases, educational implications are clearly drawn.

The studies present a nice balance between objective measures of the organic effects of music, such as motor phenomena induced by certain selections, and introspective accounts of effects on thought, feeling, and action. On the whole, this series of studies should give the reader a fair sampling of the kind of contemporary inquiry which is being carried on concerning the effects of music. The book should be useful alike to students and teachers of musical science, to those who are interested in the psychology of æsthetics, as well as the reader with a non-technical, personal interest in his own reactions to music.

ANN SHUMAKER.

The Lincoln School of Teachers College.

THE PSYCHOLOGY OF ABNORMAL PEOPLE

With Educational Applications, by John J. B. Morgan. New York: Longmans, Green and Co., 1928. Pp. IX + 627.

Psychiatrize education and psychology! Variously expressed this has been the demand of leaders and followers in the mental hygiene movement. More so than any other single book which has come to the attention of the reviewer, Dr. Morgan's latest, "The Psychology of Abnormal People with Educational Applications" satisfies this described need. The book is a veritable "educational psychiatry." As he sees it, the kinks and twists—the peculiarities of human behavior—are largely the result of training and by far the largest number of problems referred to behavior clinics (the director of which is usually an M.D.) are not, strictly speaking, medical but are psychological

and educational. This general attitude is revealed in the following quotation from the Preface: "If we find that we have learned peculiar mental trends, the way to overcome them is naturally through the process of relearning. If we come to spell badly, we do not correct such a defect by recalling the fact that we fell from a tree in childhood or by consulting a physician to examine our bodies—we take lessons in spelling. In like manner, if we have an abnormal fear, if we have false beliefs, or if we hate all our neighbors, we do not correct such a condition by blaming our defection on some disorder of our stomach or heart, but by educating ourselves into a different attitude. A great part of mental hygiene is education."

Convinced that "of all our possessions we value none more highly than our mental integrity" and that "mental health is furthered greatly by a frank study of mental abnormality," he attempts in a straightforward and aggressive manner to present the most significant contributions of psychiatry, psychopathology, and abnormal psychology—all well integrated—which throw light on the nature and causes of the adjustments, lack of adjustments, and maladjustments of actual living people confronted by the realities of life. At no time does the author lose himself in a world of abstractions. He is, in the words of James, "on speaking terms with the universe." And, more particularly, of the people in it! His primary interest, that is to say, is not abnormal psychology as such, but is the psychology of the abnormals. It is this slant which distinguishes it from another excellent book, a recent contribution to the field, Professor Conklin's "Principles of Abnormal Psychology." Professor Conklin writes "as a psychologist for students of psychology." The emphasis, therefore, is on abnormal behavior, not treatment; the point of view is that of the scientist, not the practioner; and the attitude, thus, more detached. Dr. Morgan, on the other hand, writes as a psycho-clinician. To him science is a method of solving problems. He does not burden the reader with such concepts as the "tripartite nature of the normal as well as abnormal psyche" or with the "intrapsychic censor." Whether, *à la Freud*, all ego stirrings are tinged with sexuality or, *à la Adler*, all sexual feelings are tinged with egotism—these are not dilemmas to him. The problems he concerns himself with are more vital and his method of attack more practical. He considers dreams not to demonstrate the validity of any forced interpretation of them, but for the light they throw on the nature of the dreamer. Similarly, his "purpose in studying hypnosis is to discover what light it sheds upon the personality of the

person hypnotized." Similarly with other behavior manifestations. All problems considered radiate largely from such questions as: What satisfaction is the individual trying to attain? How did it come about that the adjustments made seem ineffective, erratic, peculiar to normals? The interest, that is, is in the patient and his attempts to satisfy his desires in known psychological situations. His thinking is never truncated. He applies only those contributions of Freud, Jung, Adler, and more so of Bleuler that he considers valid and which are applicable to his presentation.

Briefly, it can be said the materials are well selected, the presentation generally critical and always intelligible. Each chapter and each section of each chapter is prefaced by preliminary remarks evidently purposed to stimulate an interest in the subject-matter and help the student select the significant and avoid "fictitious guiding lines." Each chapter is followed by five or more projects for further study and a list of references and, wherever necessary, a glossary of important technical words. It is especially well adapted for a course which is on the rapid increase in our universities and colleges—a single course in mental hygiene and abnormal psychology. Its only competitor for this use is Pressey's "Mental Abnormality and Deficiency"; Taylor's "Readings" are more suited for the instructor than for the student. The chief difference between this book and Pressey's is that the last named contains more case studies and an abbreviated treatment of mental mechanisms whereas in Morgan's book the emphasis is on the understanding of the dynamics of human behavior in all its manifestations.

H. Meltzer.

Psychiatric Clinic, St. Louis, Mo.

An Appreciation of Morton Prince

Morton Prince and Abnormal Psychology, by W. S. Taylor. New York: D. Appleton and Co., 1928. Pp. XI + 137.

Mention the name Morton Prince to all who have ever heard the name before and ask them to respond with the first association. The most frequent response will doubtless be "dissociated personality" or "Miss Beauchamp." Dr. Prince has made many more significant contributions to abnormal psychology but these are nowhere near as well known. In the "Psychologies of '25" he is classified with

McDougall and the "Purposive Groups." As a result, a good many psychologists have so categorized him. Dr. Taylor is not at all sure that he belongs there. Morton Prince is influenced by all contributions to the field, of course. The influences and relationships are intelligently traced in the book. But Prince is critical-minded. He does his own thinking. He builds his own systems. Moreover, he—more so than such leaders of schools of psychopathology as Freud, Jung, Alder—is not only aware of the facts of general psychology but is a student of them and applies them in the interpretation of the more astonishing features of human behavior which are called abnormal. In the words of Dr. Taylor, he has made "the phenomena of abnormal psychology psychologically comprehensible."

In this little book Dr. Taylor, fresh from his extensive studies which must have preceded the preparation of his "Readings in Abnormal Psychology and Mental Hygiene" attempts a sympathetic but critical and very systematic analysis and interpretation of the contributions of Prince to the field of abnormal psychology. "The sketch," as he informs us in the introductory chapter, "is based upon Morton Prince's considerable body of writings; upon observations of students' reactions to his formulations and to the authors' points of view; upon various investigations of functional abnormalities in the light of these views; and, especially, upon recent and detailed discussions with Dr. Prince himself."

The limitations as well as the meaning and significance of Prince's work and point of view are considered by the author under the following aspects of abnormal psychology which are treated as separate chapters—some very small chapters. The aspects considered are: General Conception of the Organism; The Problem of Urges; Association, Integration; Conflict; Dissociation; Consciousness and the Subconscious; The Genesis of Functional Disorders; Types of Functional Disorders; The Basis of Psychotherapy. The concluding chapter is a succinct summary of the deficiencies as well as the positive contributions of Prince's system "compared with the range of observations and hypotheses (and speculations) that compose the psychopathology which prevails today." The copious notes at the end of the book are to the point and throw light on the content proper—the man as well as his system of concepts.

H. MELTZER.

Psychiatric Clinic, St. Louis, Mo.